# Antiquity

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## Editorial Notes

THE exposure of the Piltdown fraud has been welcomed by two very different groups of people. Human palaeontologists are glad to be rid of objects that were inconsistent with each other and with a large range of securely established facts; and those who, from envy, hatred, malice, uncharitableness, or just plain ignorance, rejoice to see 'experts' discomfited, have presumably derived much consolation. The discomfiture must be admitted, but who were the 'experts'? The last thing we would wish would be to increase the discomfiture of those, if any, who are still amongst us; but it is only just to point out that none of them were archaeologists. The only one of those concerned with the original discoveries who could possibly be so called was Dawson himself, and his claims were very thin.

The bones and teeth were supposed to have been found in a shallow deposit of gravel and to be contemporary with or older than it; they were therefore the concern of those anatomists who specialize in human palaeontology, and the gravel was the concern of geologists. Archaeology so far did not come into the picture at all, and it was quite proper that the first account should have been given at a meeting of the Geological Society. (The present writer was present at it and remembers it well). Archaeologists deal with the works of man, not with his body, and as excavators it is their business to learn how to distinguish disturbed from undisturbed soil. The question of whether the Piltdown gravel had been disturbed seems never to have been asked; even if it had, it would have been difficult to answer because at that time in England the technique of excavation was still, in spite of General Pitt-River's work, very crude. Nevertheless, though the question was not asked, it should have been; soil-study was involved, and to that extent there was an archaeological aspect of the alleged discoveries.

But the Piltdown forger was careful to avoid as far as possible burying objects at the site. All the larger objects were picked up on the tip-heaps. Only one piece of skull was found *in situ*, and it was no larger than a thumb-nail and could easily have been pushed into the vertical face of the excavation. The slab of elephant-bone was tucked into the soil under the hedge, where it was said to have been 'presumably thrown by the workmen' (or words to that effect). A minute sliver of identical bone was found in the basal seam of clay, 'indicating the original horizon of the worked slab' (or words to that effect). The famous jawbone itself was in a small hump of basal gravel which the workmen had missed by reason of the floor of the pit being flooded. These facts suggest that the forger was astute enough to realize that any considerable disturbance of the soil might be detected, and to avoid it for that reason.

This matter of soil-disturbance is of outstanding importance, as every excavator knows. It may occur in any site, whether stratified or not ; in barrows and other earthworks burrowing animals, particularly rabbits, are prime sources of trouble, and so are the ancient occupants of a site who frequently dug pits and post-holes. But—and this is the important point—the qualified excavator is fully aware of these hazards ; he is on the watch for them unceasingly all the time he is at work, and he can detect them when they occur. Usually it is not fraud that he is anxious about, but the danger of attributing to an older deposit objects which are in fact intrusive from a younger (upper) layer. Where fraud is concerned there are now ways of estimating the age of bones by treatment of the things themselves, but it is seldom possible to do this with archaeological objects. When these are in any way unusual, they can often only be dated by their associations. It is an archaeologist's business to do that, and it can only be done on the spot, not afterwards in a museum or elsewhere.

It is therefore hardly correct to say, as did a recent reviewer (*Times Literary Supplement*, 11 November, 1955, p. 675) that ' the diggings of archaeologists and the conclusions thus formed about the course of evolution have been peculiarly vulnerable to an expert who for any reason wishes to plant a find '. It is precisely in the ' diggings ' that fraudulent insertion can most easily be detected, as at Glozel, by the normal methods. That has always been so, and on all properly conducted excavations it has always been a rule that all important finds are to be left undisturbed in the soil until they have been seen and recorded by the supervisor, who then removes them himself. Naturally he looks for signs of disturbance, which he seldom fails to detect. The passage just quoted also seems to imply that, on other sites besides Piltdown, erroneous conclusions have been drawn from objects which have been planted, as alleged, by ' experts '. One would like to be told where they are ? In any case, the ' course of evolution ' has been quite firmly established in broad outline by many discoveries made both before and after the Piltdown affair, by which they are in no way invalidated.

The review from which we have quoted was of a book called *Counterfeit* (John Murray, 15s.), written by Mrs Sonia Cole, and we agree with the opinion that it is ' a pretty good one '. But we are surprised that the reviewer was at first surprised to find an archaeologist writing ' about cheats '. He discovered why later ; it was because our ' diggings ' are so vulnerable thereto ! If he were more familiar with archaeology he would know that, quite apart from ' cheats ', archaeology *is* detection, and inference. Its methods are those of Sherlock Holmes applied to man's past. So it is merely an extension of the normal archaeological practice when the things to be detected are not past but present (fraudulent) activities ; and if anyone outside Scotland Yard is by training fitted to write a book about ' cheats ' it is an archaeologist. It only shows how little archaeology is really understood, in spite of its popularity. Where documents are involved it is a historian's job, or more strictly an archivist's. In both subjects a knowledge of the relative values of evidence is required, and that is why, when properly taught, they are so useful in the modern world. Those who have benefited by such training are for that reason better able to evaluate the contents of newspapers and their advertisements or, as members of a jury, the evidence of witnesses.

# EDITORIAL NOTES

The reviewing of books is one of the functions of ANTIQUITY. In common with other journals we cannot usually review periodicals, though we may do so occasionally. In practice the choice is limited to those we receive. We sometimes write and ask for review copies of what seem to be important publications, and generally receive them in response. But there are some which we cannot get, chiefly those published or subsidized by foreign governments; that is unfortunate for several reasons. It is alleged that these, being subsidized, do not need the publicity of reviews—in other words, it does not matter whether they are sold or remain stacked in heaps in the office of the publisher. The taxpayers, at whose expense they are printed, might think otherwise, and in this country the Stationery Office, the Government publisher, pursues a much more enlightened policy. The result is that many archaeological books of basic importance, such as the final and definitive reports of large-scale government-sponsored excavations, escape that informed criticism which is the life-blood of all scientific work, and which as everyone knows they often badly need. Some of course are very good; others on the other hand are produced in a wasteful and extravagant format, with huge margins, masses of over-reduced half-tone illustrations, which, even when made from good photographs (which is exceptional), are often uninformative or merely inadequate substitutes for more plans, sections and drawings which require so much more work to produce. A plan or drawing which should be drawn for reduction has to be reproduced full size as a tiresome folder because the draughtsman did not allow for this. The whole confused mass is just thrown at the reader, who has to sort it out as best he can. Too often also the excavations described are themselves badly in need of criticism. In countries where professional archaeology is paramount (as it is in most southern countries) mordant criticism is stilled, for those inside are too closely involved to make it. The result is stagnation and the acceptance of standards of work and publication which are out-of-date elsewhere. In scientific work the rule of non-interference in the domestic affairs of other countries does not apply; we all suffer because our interests are essentially international.

With this number of ANTIQUITY we welcome a large accession of new readers, many of them in the United States of America. We hope that their first impressions will be favourable, and we feel quite confident because we know that this is a good number, with something of interest for everyone. We have some good stuff for the June number; an article on the Earliest Firemakers, by Dr Oakley; others on Early Greek Roads, Upland Houses, and the Archaeology of the Philippines. In another article we shall publish evidence to prove that coal was burnt in the Old Stone Age, some 40,000 years ago. We welcome suggestions (the note on Swanscombe in this number is the result of one such); and we also welcome criticisms, and try to act upon them.

Many of our new American readers came to us through an advertisement exchanged with the *Scientific American*, a monthly publication covering a wider range than our own. We wish to take this opportunity of recommending our readers to get the *Scientific American*, whose advertisement appears at the end. We have ourselves enjoyed reading it for many years; it contains popular but authoritative articles on the latest developments in physics, biology, astronomy and archaeology, and does for science as a whole what we try to do for archaeology.

# The Earliest Tool-makers

by Kenneth Oakley

MAN has been defined in many ways, as the talking animal, the religious animal, and so on, but the most satisfactory definition from the scientific point of view is probably Man the Tool-maker. One might ask, why bother to define man, considering his uniqueness and self-evident characteristics? But from the standpoint of students of evolution, particularly those concerned with the interpretation of fossil remains of early man and his possible ancestors, to define man is of practical importance.

If one compares the anatomy of man with that of other mammals, it is clear that man is most closely related to the great apes. This implies that men and apes have descended from a common ancestral stock. To some extent we can deduce the characteristics of that stock by noting features in which apes and men resemble one another and more particularly features in which they differ. Thus apes have much smaller brains than men, and since increase in brain-size is progressive, we infer that in the common ancestors the brain was much smaller than in man, and no larger than in apes today. Men habitually walk upright on two legs, and these are longer than the arms; apes on the contrary live in forests and are accustomed to move about by swinging from bough to bough—their arms are longer than their legs. But it is probable that the common ancestors of apes and men were not specialized either for swinging from boughs or for walking upright in the open; their legs and arms were presumably of nearly equal length as in the agile four-footed monkeys.

Remains of monkey-like apes that lived about thirty million years ago have in fact been found in Kenya, and in many ways they conform closely to the postulated common ancestors of men and the modern great apes. These Early Miocene creatures—named *Proconsul*—were unspecialized, and associated fossils show that they lived in country that consisted of corridors of woodland separated by open bushy grassland. Although accustomed to living in and among the trees, there are anatomical indications that they could run in the open and even perhaps scuttle on two legs when it became necessary to cross an area of tall grass between one belt of trees and another.

It is evident that in parts of Africa where this mixed environment gave place to continuous forest, apes of the unspecialized *Proconsul* type might gradually have become adapted to life in the trees, thus evolving into gorillas or chimpanzees. But where on the other hand the country remained more open, those monkey-like apes that had acquired the habit of occasionally scuttling on two legs would have been well fitted to survive even complete deforestation of the region. And it was probably thus—through an environmental crisis—that the evolution of man began.

But when examining the fossil evidence that may throw light on this crucial stage in evolution one is immediately conscious of the need to define man. It would obviously be inappropriate to apply the term human to every link in the complex chain leading from *Proconsul*-like creatures of thirty million years ago, to the emergence of *Homo sapiens* less than a quarter of a million years ago. As the divergence of the ancestors of man from the forest-bound apes was fundamentally connected with beginning to walk on two legs, one might expect changes to occur in the structure of the hip-girdle before the brain had expanded to typically human proportions. One might expect too that as

the teeth and jaws of men differ from those of apes in a number of respects, these would have begun to change in connection with adaptation to the new mode of life, which may well have involved a modification of diet, and manner of feeding, with greater use of the hands.

If we were to find remains of ape-like creatures that had teeth and jaws similar to ours, and hip-girdles of the type developed for walking erect, but with brains no larger than those of gorillas, should we call them men ?

This is the problem posed by the remains of *Australopithecus* found in ancient cave deposits in the Transvaal (PLATE I). The Australopithecines had a number of so-called human features but because their brains were smaller than any acknowledged human beings, some anthropologists regard them as aberrant apes with no claims to be more closely related to us than the other great apes. However, as they had teeth and jaws of a type found in man but not in modern apes, and since, judging from their hip-bones, they walked upright, it is really no more logical to call them ' apes ' than to call them ' men '. This is a case where the use of everyday terms is apt to be more confusing than use of technical language. It is agreed by the majority of anthropologists that the Australopithecines were either part of, or very close to, the line of evolution that led to man. If we call all the members of this line including man himself the *Hominidae*, in contrast to the line leading to the modern apes called the *Pongidae*, we can express the probable status of the Australopithecines by saying that they belong to the pre-human part of the family *Hominidae*.

How then should we define the *human* stage ? If we use the term human with its accepted connotation, we imply a being with a capacity for rational thought. Since the largest adult ape's brain is smaller than the smallest acknowledged human brain, brain-size might be used as the criterion of humanity. This rubicon might be between seven hundred and eight hundred cubic centimetres. But this is not very satisfactory when one is dealing with borderline cases, because, even among our own species, there is no close correlation between mental ability and brain size. We have to allow for the possibility that the brain of *Australopithecus*, at most only 750 cubic centimetres in the adult, had an exceptionally well-organized cerebral cortex.

It has been suggested that ability to talk and to make tools are better criteria of humanity than brain-size. We cannot know anything about an Australopithecine's powers of communication ; but when one remembers that children of our own species are beginning to talk at the age of two, with brains that have only reached eighty per cent of the adult size (minimum say 830 cubic centimetres), it is conceivable that brains no larger than those of *Australopithecus* are in fact capable of functioning at the level required for speech. Ability to make tools is a criterion that can be applied more easily to extinct types, and it is for that reason that the definition ' Man the Tool-maker ' appears to be the most useful one. But to what extent is tool-making fundamentally human ? There are a number of examples of animals *using* tools, but only man regularly *makes* them. Chimpanzees in captivity have been known to make tools occasionally, yet only with a visible reward as incentive ; for example, one of those studied by Koehler fitted a stick into a bamboo rod in order to reach bananas that were otherwise inaccessible. Apes do not make tools in readiness for an imagined eventuality in the way that men do. Systematic tool-making, even of the simplest kind, indicates foresight based on memory ; and it indicates tradition, which is another vital characteristic of human society.

Australopithecines have generally been regarded as sub-human because of their small brain size, but this view would be invalidated if it could be shown that they had been tool-makers. In 1953 evidence came to light indicating that pebble-tools were made in

South Africa during the period of the Australopithecines, but until recently none had been claimed as occurring with their remains. Last year, however, some possible pebble-tools were reported from one of the Australopithecine sites. They were exhibited by Professor C. van Riet Lowe, at the Pan-African Congress on Prehistory which took place in Livingstone last July. The specimens in question are crudely chipped slabs or pebbles, mostly of dolomite (PLATE II), a few of quartzite, and they were found by Mr C. K. Brain, in the cave at the Makapan Limeworks in a deposit of gravel immediately above the main layer containing bones of *Australopithecus*. One fragment of an Australopithecine jaw has now been discovered *in* this gravel, which was apparently washed into the cave through flooding of the adjacent valley. There are two problems to be decided : were these tool-like pebbles in the gravel chipped by accidental agencies, or do they show undoubted design? If they are artifacts were they made by *Australopithecus* ? Many of us came away from the Congress feeling that a question-mark must still be placed against the particular chipped stones so far found at Makapan. Fortunately the greater part of the gravel bed remains to be excavated.

Professor Raymond Dart, who described the first *Australopithecus* skull found at Taung in Bechuanaland in 1924, claims that there is evidence that *Australopithecus* was a regular tool-*user*. In a cave deposit identical with that which contained the original skull there were numerous baboon skulls, seventy per cent of which have depressed fractures of uniform size, and approximately constant position on the crown, as though they had been produced by a well-aimed blow with a weapon. At the Makapan cave quantities of broken animal bones were found in association with *Australopithecus*. And the bones appear selected—hundreds of pieces of antelope skull, jaw and leg bones, but relatively few vertebrae. One school of thought attributes all this to the activities of carnivores, particularly hyena, but Dart and his associates recently excavated a lair of one of the living species of hyena (*Crocuta*) and did not find any broken bones in it ; and this has convinced them that the bones at Makapan limeworks were introduced into the cave by *Australopithecus*, and that some had been used as weapons. Paradoxically, if this view were accepted it would tell against the theory that *Australopithecus* made the supposed pebble-tools, for none of these was found in the main bed containing the bones.

It is of course highly likely that pre-human hominids were semi-carnivorous and that they made use of stones, sticks and bones as ready-to-hand weapons and tools ; but to prove this is difficult. And it is going to be almost as difficult to prove or disprove the theory that Australopithecines had advanced to the stage of systematic tool-making. The absence of stone implements from layers containing their remains cannot be held to disprove the theory that they were tool-makers, so long as it is possible that the remains represent the food débris of cave-dwelling carnivores.

Only when an undoubted living-place of a species of *Australopithecus* is discovered will it be possible to answer the question of whether that species was a maker of pebble-tools. Yet undoubted pebble-tools have been found in contemporaneous deposits (e.g. in Basal Older Gravels of the Vaal at Harrisdale). If Australopithecines were not the makers of these implements, then some more advanced hominid was evidently living contemporaneously with them. That is of course by no means impossible. Two genera of apes are living in the Congo today, why not two genera of hominids in the Transvaal in the early Pleistocene period ? Dr John Robinson of the Transvaal Museum claims that there is direct evidence of this at the site known as Swartkrans. A breccia there has yielded remains of over thirty individuals of a species of *Australopithecus* (' *Paranthropus* '), and in addition two jaws that have been ascribed to a separate genus, *Telanthropus*, considered by Robinson to be in advance of the associated *Australopithecine*.

A related question that was discussed at the Pan-African Congress was the geological antiquity of the Australopithecines. It now seems fairly certain that none of the known examples is older than the end of the first pluvial of the Pleistocene period—about half a million years ago—and corresponding approximately to the end of the first glaciation in Europe. Geological evidence suggests also that the oldest examples of undoubted man, *Pithecanthropus* of Java, date from the same time. We know that at least one species of *Pithecanthropus*, that of Pekin, was a tool-maker and user of fire, and his brain-size ranged from about 770 to 1300 cubic centimetres, thus actually overlapping the brain size in *Homo sapiens*. So the human status of *Pithecanthropus* is confirmed on all counts, and yet it is proving quite difficult to distinguish some of the more primitive variants of this genus from *Australopithecus*.

Because the known members of *Australopithecus* are too late geologically to be the actual ancestors of *Pithecanthropus* of Java and Pekin, this does not mean that earlier representatives of the same genus may not have been ancestral. It is a common phenomenon in evolution for a given stock to become split up, and for one part to remain relatively static and for the other part (usually that which has moved into new territory) to undergo more rapid evolution.

The stone implements of *Pithecanthropus* of Pekin were rough flakes and crude choppers seemingly not far removed from the basic tool-making tradition represented by the pebble-tool industries of Africa. In Africa pebble-tool culture gave place to one in which the predominant tools were tongue-shaped implements with sharp cutting edges known as hand-axes. These were the first standardized tools, and doubtless served many uses, but chiefly as a hunter's knife. It is surely significant that at all the oldest known living sites of early man, both in Africa and Asia, there is an abundance of broken meat bones. Man, in marked contrast to apes, is essentially a hunter. Hand-axes have been found in deposits ranging in date from more than a quarter of a million years ago to perhaps 50,000 years ago, and they are found throughout Africa, on to the tip of the Cape of Good Hope itself (where they are made of Table Mountain Sandstone) and throughout most of South-west Asia and North-west Europe where they are mostly of flint or chert. The question of what type of man, or types of men, made hand-axes has been one of the abiding mysteries of prehistory.

The rarity of remains of man dating from the huge stretch of time represented by the hand-axe period is probably due to the fact that hominids stand little chance of being fossilized unless they are the prey of cave-dwelling carnivores, as the Australopithecines may have been, or combine dwelling in caves with cannibalism as Pekin Man seems to have done, or bury their dead as did the later Palaeolithic races.

For many years there has been a theory that hand-axe makers were the first men of our own species, *Homo sapiens*, living contemporaneously with the more bestial *Pithecanthropus* of Asia. This was supported by several pieces of rather inconclusive evidence: a scrap of diseased human jawbone with apparent chin, found by Dr L. S. B. Leakey at Kanam in Kenya along with advanced pebble-tools; a smooth-browed type of skull found with hand-axes in disturbed beds at Kanjera also in Kenya; and thirdly the back and top of a skull found with hand-axes at Swanscombe in Kent. (A third bone of this skull, the right parietal, was found last July). In so far as it is preserved the Swanscombe skull is not distinguishable from some skulls of modern man, yet its antiquity is assured by the same fluorine, nitrogen and radioactivity tests that proved the modernity of the Piltdown jaw. The doubt about the Swanscombe skull is whether the still missing frontal bone would have had a projecting ridge like the earlier *Pithecanthropus* or later

Neanderthal skulls, and whether the jaw would have been chinless or not. But the part that is preserved is distinct from either of those types.

New light has been thrown on this question by a most important discovery recently made at Ternifine in Algeria by Professor C. Arambourg and some of his staff from the Paris Natural History Museum. They have found three well-preserved human jaws in deposits containing Acheulian hand-axes of a type more primitive than those of Swanscombe, and associated with butchered remains of elephant, horse, hippopotamus and sabre-tooth tiger, which had evidently been hunted at a water-hole near the margin of the lake in which the deposits had been laid down, probably more than a quarter of a million years ago. The jaws have receding chins and the ascending rami are exceptionally wide, as in the Heidelberg jaw, which is slightly older. The teeth are remarkably similar to those of *Pithecanthropus* of Pekin. Ternifine Man has been described under a new name, *Atlanthropus*, but this is almost a confession of ignorance because it is impossible to be sure of the precise status of a hominid until the greater part of a braincase is found. So far only a small piece of parietal bone of the braincase of *Atlanthropus* has come to light. On the evidence available there seems no reason for excluding these early Acheulian hand-axe makers from the *Pithecanthropus* group.

Other skeletal remains attributed to hand-axe man are of strangely contrasted types. The Kanjera skulls are thick like the Swanscombe skull, and in one the forehead is preserved and shows no brow ridge. The associated hand-axes are later in type than those of Ternifine, and similar in stage to those of Swanscombe. A skull almost identical with the much later Rhodesian or Broken Hill skull, distinguished by heavy brow ridges, was found in 1953 near Saldanha Bay in South Africa, in a situation that left no doubt that it came from a deposit containing very late (Fauresmith) hand-axes.

It begins to look as though Africa and Europe were inhabited during the hand-axe period by several hominid groups undergoing evolution at different rates; Ternifine Man may have been among the most primitive, but by the middle of the period some groups had practically attained the status of *Homo sapiens*. Before the end of the period various side-lines had branched off from this stock leading to aberrant species of *Homo* such as Neanderthal Man in Europe and Rhodesian Man in Southern Africa.

Now to sum up. It seems that Africa was the evolutionary centre of the Primates; that the great apes and men evolved from a common ancestral stock of monkey-like creatures such as *Proconsul* which lived in East Africa about thirty million years ago; that the Hominid branch originated through one group of these monkey-like apes becoming adapted to living in open country, walking erect on two legs, and supplementing their teeth by the use of ready-to-hand tools and weapons when a changing environment forced them to become meat-eaters. The stage at which erect gait was attained and when tool-using began is probably represented by *Australopithecus*. The new mode of life may have reacted favourably on the developing brain; at any rate the next evolutionary stage is represented by species of *Pithecanthropus*, with large brains capable of functioning at the level of integration required for systematic tool-making. These first men existed somewhere between half a million and a million years ago, according to current estimates of an indirect kind. They showed great variability, and some stocks evolved faster than others in the direction of *Homo sapiens*. The dating of the earliest representatives of our own species in Africa is still controversial: estimates vary from 50,000 years to over 250,000 years; but certainly it was only within the last 10,000 or 15,000 years that Negroes, Bushmen and other specialized types of our species began to emerge.

# Athens in the Late Bronze Age

by Oscar Broneer

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## I

THE Athenians of the classical era were deeply conscious of the fact that the history of their city was different from that of the rest of Greece. They were the autochthonous settlers of the land, and their orators and writers kept forever reminding them that Athens and Attica were not subdued when the Dorian invaders gained possession of most of the Peloponnesus at the end of the Bronze Age. Was this an empty boast, the kind of historical error that Thucydides (I, 20) attributes to a people's readiness to accept uncritically the old traditions about their own country or those of others? The historian (I, 2) makes it clear that he himself believed in the tradition that Attica was the original home of the Athenians of his day, and he found an explanation for this phenomenon in the poverty of the soil which made the conquerors pass by Attica for richer sections of the country. Archaeological research has confirmed Thucydides' conclusions.

Largely because Athens escaped the destruction visited upon the cities of the Peloponnesus, the Bronze Age remains of the city are comparatively meagre. From the time of its first settlement in the Neolithic Age down to the present day, a period of some five thousand years, the site of Athens has been continuously occupied, and in each era the monuments of the past have been sacrificed to make room for new structures. For the period before the middle of the second millennium B.C. the remains are scant but sufficient to show an unbroken record of occupation. Beginning with Late Helladic III, *ca.* 1400 B.C. the material sources of information become somewhat more abundant, but by comparison with the great Bronze Age sites of the Peloponnesus they are meagre indeed; and this is still true after the important discoveries in Athens within the last quarter of a century. Yet, if we search the records for all the information they can give, the picture we obtain is both homogeneous and convincing. It will be convenient to begin our description with the Acropolis, which, in the words of Thucydides (II, 15), was the city, *polis*, before the creation of a united Attica under Theseus.

Very little is preserved of the king's palace. The two column bases within the foundations for the archaic Temple of Athena were very likely part of the 'well-built house of Erechtheus' (Od. VII, 81), but only one was, when discovered, clearly in its original position[1], so that, even the exact orientation of the megaron must be left in doubt. Some of the foundations to the east and north of the Erechtheion probably belonged to

[1] The information has often been given that both bases were found *in situ*; Lester B. Holland, *A.J.A.* XXVIII, 1924, p. 162; XLIII, 1939, p. 289; Judeich, *Topographie von Athen*, p. 259; and *cf.* Kavvadias and Kawerau, Ἀνασκαφὴ τῆς Ἀκροπόλεως, p. 84. I have heard Dörpfeld in his Acropolis lectures question whether the northern column basis is in its original position, and the present underpinning would indicate that it has been disturbed. Its top is 0.076 m. above that of the southern base, an unlikely discrepancy if both bases were *in situ*. The question is discussed at some length by James M. Paton, *Erechtheion*, pp. 427 ff.

the palace complex, but they cannot be part of the megaron proper and are not sufficient for a convincing restoration of any part of the building. We can only guess, by comparing these scanty remains with the palaces of Tiryns, Mycenae, and now with the palace of Nestor at Pylos, what the Athenian Acropolis may have been like in the time of Theseus and his predecessors.

The citadel was surrounded by a wall of Cyclopean masonry, the line of which can be traced for most of its length (FIG. 1). One section, south of the Propylaea, is preserved to a height of about ten feet (PLATE III). The wall followed the irregular line of the comparatively level area on top of the hill far more closely than does the existing wall of Classical and Medieval times. The Athenians ascribed the construction of the wall to the Pelasgians, who, as payment for their labours, received a tract of land below the Hymettos, or, according to another tradition, at the foot of the Acropolis. Whatever truth lies in this story—and it may have been invented to account for the name—the fortification was known as the Pelasgikon (*teichos*). Another form of the name, Pelargikon ('stork wall'), also occurs. Both forms apply to the wall on the Acropolis and to an area of uncertain extent on the north and west slopes, which was inviolate and was largely occupied with primitive cult places. This area may have been a late edition to the original citadel on top of the hill.

Only at two points was the Acropolis slope gentle enough for an approach from below. The only obvious place for a main entrance was at the west end, where we must assume that a monumental gateway was built, comparable to the Lions Gate at Mycenae. Perhaps there was also at a higher level an inner entrance-way like the principal propylon at Tiryns. No foundations for these structures are visible today, but the Mnesikleian Propylaea may conceal some traces of them.

The prominent bastion supporting the Temple of Athena Nike is a creation of Mycenaean times. This had long been suspected, but it was not revealed until 1936, when it became necessary to demolish the marble temple prior to its second modern reconstruction. Before the middle of the 6th century an altar and probably a cult statue were set up, perhaps for the celebration of the Panathenaia in 566 B.C.[2]. But the cult seems to go back to a still earlier period, even to Mycenaean times. In the 5th century B.C. the bastion was encased with a poros wall which conceals the Cyclopean masonry. Two conspicuous niches in the west face of the classical wall have long puzzled the archaeologists, and it is now certain that they were made to take the place of similar niches in the Cyclopean bastion. The late Gabriel Welter conjectured that these niches originally served some purpose in connection with one of many successive gates that led to the main Propylon of Mycenaean times[3]. His reconstruction, tentative and conjectural, was an attempt to explain the name Enneapylon, which some ancient authorities use as the name of a gateway in pre-classical times. There is no parallel in Mycenaean fortifications for a long winding approach, such as Welter proposed, with eight gates in succession before the principal propylon is reached. At Tiryns, the only comparable case, there are three gates, only one of which could offer an effective obstruction to an enemy. An approach with nine gates would have been an intolerable hindrance to communication in peace time and probably unnecessary and ineffective as a measure of defence. The niches in the pre-classical wall are rather to be connected with some cult, for which there is other evidence, as yet unpublished.

The earliest reference to the 'Nine Gates' can be traced to Polemon, who wrote in the 2nd century B.C., but the word Enneapylon also occurs as an adjective with

[2] Anton Raubitschek, *Dedications from the Athenian Acropolis*, p. 359.

[3] *Arch. Anz.*, 1939, pp. 1 ff.

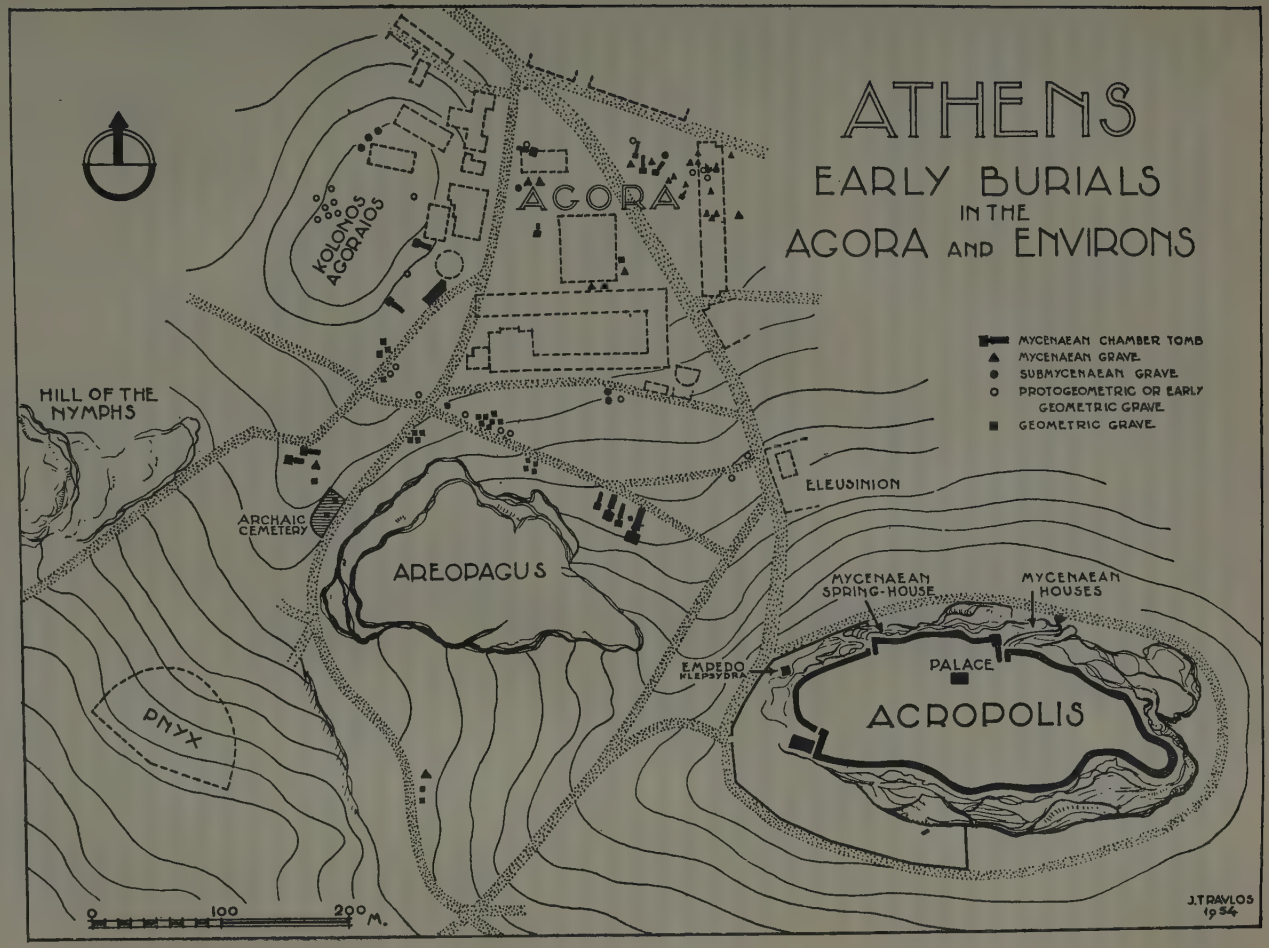


FIG. 1. MYCENAEAN ATHENS

Pelargikon[4]. If Enneapylon is a descriptive architectural term rather than an exaggeration formed on the sacred number nine, it probably applied to the lower extension of the Acropolis which, like the wall itself, came to be called Pelargikon or Pelasgikon.

In addition to the main approach from the west there was a postern gate communicating with the north and east slopes of the hill. The early foundations in this part of the Acropolis indicate the existence of towers[5] flanking a single opening, which was approached by a path with flights of steps inserted where the incline was steepest. Two periods of construction are indicated, the first of which may be as early as the 15th century B.C. In the final phase of the fortification, to which we shall presently return, the gate was closed and the path and stairs outside the wall became buried under small, poorly constructed houses (PLATE IV). At a later time these dwellings were abandoned, apparently as a result of a sudden threat to the city, and the inhabitants presumably took refuge within the walls. The pottery left standing on the floors as they fled from their houses shows that this took place in the last quarter of the 13th century B.C. (L.H. III B). Whatever caused the sudden abandonment of the north slope settlement, the houses were never again rebuilt[6].

An important feature in the fortification of the Acropolis was the construction of an artificial fountain with access from the citadel. The engineers charged with this task displayed great ingenuity and a remarkable knowledge of the geological condition of the rock. Their scheme for providing the citadel with water is far superior to the system used at Mycenae or at Tiryns, where the sources lay outside the fortified areas. In Athens they took advantage of a deep cleft in the rock on the north side of the citadel. From earlier experiences with the water problem on the north-west slope they probably knew the approximate depth at which water was obtainable. In the natural cleft in the rock they sank a shaft to a depth of about 120 feet below the Acropolis level and wide enough for the construction of a stairway[7]. The first two flights below the Acropolis were of wood, five more had stone steps supported on an underpinning of reinforced rubble masonry, and wooden cross ties anchored the stairway to the almost vertical side of the rock. These seven flights descended to within 25 feet of the water level. Below the bottom step there is a vertical shaft, and here the water was probably raised by means of jars let down with a rope.

The flow of water was copious during the rainy season and was probably sufficient for the most pressing needs throughout the rainless months of summer. The construction of the fountain was no easy undertaking; presumably it was dictated by dire necessity. Under peaceful conditions they could use the fountain of Klepsydra, originally called Empedo, which was within easy reach of the Acropolis and could be approached by an outside path[8]. But it was outside the wall, and the slope was too rocky for the construction of a covered stairway like that leading to the fountain at Mycenae. And yet, a water supply that could not be intercepted by an attacking foe was an essential part of the defence programme.

The nature of the construction, with wooden beams used for reinforcement, indicates that the fountain was made in haste to meet an emergency. The builders must have

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[4] See references in Ernst Curtius, *Stadtgeschichte von Athen*, p. LXXVI, and Sam Wide, *Pomerium och Pelargikon*, pp. 26–8.

[5] Lester B. Holland, *A.J.A.* XXVIII, 1924, pp. 143 ff.

[6] *Hesperia* II, 1933, pp. 351 ff.; IV, 1935, pp. 109 ff.

[7] *Hesperia* VIII, 1939, pp. 317 ff.

[8] *Hesperia* XII, 1943, pp. 201 ff.

known that the timber would soon decay in the damp underground descent. When the wooden framework gave way the stairway crashed down (PLATE V) and the débris filled much of the shaft. It is possible that an earthquake hastened the destruction, but severe earthquakes are not common in Athens and the stairway itself cannot in any case have outlasted the wooden reinforcement. The fountain was never re-opened, and the passage was subsequently used as a dump. We are led to conclude that the emergency had passed by the time the stairway collapsed, and that it was then unnecessary to keep the fountain open since the water from the Klepsydra could be reached safely and more conveniently. The pottery broken and lost in the water, or left standing on the side of the stairs at the time of use, shows that the underground fountain was constructed only a short time before the houses were abandoned on the northeast slope. This took place during L.H. III B times, and not long before L.H. III C (Granary Style) pottery came into use. Some potsherds found in the bastion beneath the Athena Nike Temple, and other samples extracted from the fill of the Pelasgian wall on the Acropolis, are of the same date and show clearly that the fortification was largely rebuilt at the time that the fountain was constructed.

It is obvious that events moved fast in the second half of the 13th century B.C. The rebuilding of the wall, the construction of the bastion to strengthen the western approach, the closing of the postern gate which made the outer slope available for squatters, and the construction of the fountain, all took place within a brief span of time, probably less than half a century. It is likely that at this time, too, the fortified area of the Acropolis—as in Tiryns and Mycenae—was enlarged by the addition of the Pelargikon to which the name Enneapylon was added. At the end of this period, the houses on the north-east slope were abruptly abandoned; and somewhat later the stairway to the fountain collapsed and the shaft became filled with débris. If there were no other evidence than that offered by the material remains we should be forced to conclude that at the end of the 13th century the city prepared to meet some outside threat, which was either successfully warded off or failed to materialize.

At this stage in the history of the city the evidence from the Acropolis itself becomes less abundant. No walls or foundations of buildings can be attributed to the period following the destruction of the fountain. That the hill continued to be occupied is shown by some sub-Mycenaean and Protogeometric pottery from the fill of the fountain shaft and from the Acropolis itself.

The chief evidence for the history of this era comes from the cemeteries on the north and west slopes of the Areopagos, in the area occupied by the Agora of Classical times, and along the eastern slope of Kolonos hill.

The earliest of the Mycenaean burials in Athens contained L.H. I pottery, but most of them are dated in L.H. III B (*ca.* 1300–1200 B.C.). They are of two distinct varieties: (*a*) chamber tombs, of a kind found at most Mycenaean sites in Greece, and (*b*) pit graves (FIG. 1). The former, which were mostly family graves, were sealed after each interment and reopened for new burials. These tombs are comparatively rich in votive offerings, and it is a safe assumption that the occupants belonged to the ruling aristocracy. One such tomb described below was probably made for some member of the royal family. The pit graves, designed for single burials and not reopened after the original interment, are usually more modest in their furnishing.

The largest and richest of the chamber tombs was found in 1939 on the lower north slope of the Areopagos.[9] Although its roof had caved in and most of its contents

[9] *Hesperia* IX, 1940, pp. 274 ff.

had been removed, and pits of later periods had been sunk into its dromos and chamber, the offerings found beneath the fallen rock are so rich as to justify the excavators' conjecture that the occupant was a royal personage. The skeleton was missing but the type of objects left in the tomb shows that it was a woman. The most noteworthy of the grave offerings are two ivory boxes, the larger of which is a particularly fine example of ivory carving (PLATE VI). The sides and the lid are decorated with figures of griffons attacking deer. The frightened victims, pursued by the winged monsters, leap over rocks and bushes in a scene which, in brutal violence and rapidity of motion, is unexcelled among ivory carvings of this period. The smaller box is covered with stylized marine motifs. Nearly one hundred gold ornaments—rosettes, double spirals, and a shell design—were found on the floor of the chamber. The pottery from the grave included four three-handled jars, decorated with a variety of designs, a monochrome spouted jug, two squat jars (usually called alabastra), and one undecorated amphora. A bronze ladle, a mirror, hairpins and some smaller metal objects complete the inventory of finds from the chamber.

The princess had been buried in a shaft in one corner of the rectangular chamber and a single slab laid over her grave. The stone was found lying on the side of the empty shaft. After the collapse of the roof, which left the interior of the chamber partly exposed, the body and the most valuable of the tomb offerings had been removed.

The ' Princess Tomb ', described above, is the richest of the chamber tombs in the Agora. Because of the damage to the roof it was never reopened for later interments, and in this respect it is not a typical example. In one large chamber-tomb found in 1951[10] within the foundations of the Temple of Ares, at least thirteen bodies, possibly as many as seventeen, had been interred in the course of two centuries. With the skeletons were found 27 vases, a bronze dagger, nine arrowheads of bronze and obsidian, and a few other objects. The offerings show that at least one of its occupants had been a warrior. The earliest of the vases is an undecorated goblet of L.H. II ware, the latest probably reach down into the 12th century (PLATE VII, A). The use of this single tomb spans nearly the whole of that period of Athenian history with which this article is concerned.

The pit graves represent a simpler form of burial, and usually contain a single skeleton. They can hardly have been the graves of the poorest classes of citizens, who probably had their own cemetery distinct from those of the ruling class. One of the earliest and the richest of the pit graves,[11] dated in L.H. III A, contained ten vases, a necklace, a gold pendant, a comb and an ivory pin. In some of the poorer graves of this class a single vase accompanies the skeleton.

The area that later became the Agora was in use as a burial ground for several centuries before the end of the Bronze Age, and it continued to serve the same purpose for nearly six more centuries. Although comparatively few burials have been discovered within this small section of the ancient city, there is no significant gap in the chronological sequence of the series (PLATE VII). The information supplied by the burials in the Agora region is further supplemented by the extensive Dipylon cemetery.[12] Here were found graves of the sub-Mycenaean period and of the early Iron Age, and these are followed by burials of subsequent times in an unbroken sequence down through the classical era.

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[10] *Hesperia* XXI, 1952, p. 107, pl. 26a.

[11] Ibid., pp. 107, pl. 26c.

[12] Kraiker and Kübler, *Kerameikos* I, plates I ff. ; George Karo, *An Attic Cemetery*, pp. 5 ff.

## II

The bulk of the Mycenaean pottery from the Acropolis and from the cemeteries belongs to well-known categories. Shapes and decorative patterns on vases, wherever found, are so much alike, that little progress has been made to distinguish such local variations as doubtless do exist. The more artistic examples are probably importations from the great ceramic centres in the Peloponnesus, and local imitations made in Athens as in other parts of Greece follow so closely the originals as to make the two virtually indistinguishable. Such uniformity in ceramic products testifies to a lively interchange of goods and ideas and this, in turn, presupposes peaceful relations and political tranquillity.

The pattern of uniformity begins to break up in the period of L.H. III B. Among the late vases of this period several new shapes and techniques make their appearance in Athens (PLATE VII, B)[13]. Local products predominate, which gradually show less dependence upon the imported types. Before the end of the Bronze Age Athens had made progress in the development of its own ceramic industry which was to flourish so conspicuously in the subsequent centuries.

The uninterrupted existence and growth of Athens from the late Helladic times into the early Iron Age contrasts remarkably with the experience of the cities in the Argolis during the same period. The major sites, Mycenae, Tiryns, Prosymna, and Berbati, were destroyed about 1200 B.C. Many sites were abandoned, at others a remnant of their population stayed on or returned later. The deterioration in the quality of pottery and works of art following the destruction is so precipitous as to indicate a serious decline in population.[14] By comparison Athens remained a populous centre, as is shown by the number of sub-Mycenaean and Protogeometric graves. The Athenian claim to continuous occupation of their city, repeated *ad nauseam* by the writers of Athens, has been shown by archaeological discoveries to be based on fact.

Yet Athens was not unaffected by the disasters that shook the Greek peninsula at the end of the 13th century. The defensive measures which can be traced on the Acropolis and which may have extendcd to the lower city must have drained the city of its resources. Such efforts could hardly fail to affect the life of the city and to be reflected in the art of the period. Furthermore, Athens can not have been wholly isolated from the rest of the world, except perhaps for a brief space of time. The general trend in the development of pottery is similar to that found elsewhere, although many of the details differ.

It is hazardous to interpret such changes in terms of political movements, especially in a period for which the evidence is admittedly meagre. Yet, on the basis of the archaeological discoveries alone, we should probably conclude that outside forces were at least partly to blame for the retardation noticeable in the late 13th and the beginning of the 12th century. It seems reasonable to connect these changes with the repeated incursions of hostile tribes which came to be known in mythology as the Return of the Herakleidai, and in history as the Dorian Invasion.

We are here confronted with a chronological dilemma which makes the history of this period peculiarly difficult. The destruction of the Mycenaean cities took place about 1200 B.C., a mere decade before the traditional date of the Trojan War and nearly

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[13] See *Hesperia* VIII, 1939, pp. 379, 391 f.; and cf. *A.J.A.* XLIV, 1940, p. 553, 558, LII, 1948, p. 109; *B.S.A.* XLII, 1947, pp. 10 ff.

[14] Cf. Martin P. Nilsson, *Historia* III, 1955, pp. 263 f. For a divergent interpretation see Alan B. Wace, *Viking*, 1954, p. 222, who would deny ' that the Mycenaean Culture decayed in the later stages of the Bronze Age '.

a century before the coming of the Dorians. Thus the prolonged invasion of the Asiatic mainland was made by the Greeks under joint command soon after the major cities participating in the expedition had been reduced to ruins or altogether abandoned. Moreover, we are forced to postulate a general destruction of the cities of Greece and a decline in civilization of which ancient writers have preserved no records, and we have little archaeological evidence for such a destruction at the time of the traditional date of the Dorian invasion. These discrepancies have led some scholars to minimize the material impact of the invasion.[15]

It is not possible within the scope of this article to review all the aspects of this problem. Because of its bearing on the interpretation of the Late Bronze Age remains from Athens, however, it seems desirable to state the problem and to suggest a solution.

The Homeric account of the war against Troy as an all-out effort under a unified command implies a degree of political and economic interdependence of the sovereign Greek cities. This condition appears to have existed in the century before 1250 B.C. In the second half of the 13th century, so far as we can judge from the material records, the structure of this cultural and industrial empire breaks up into regional units. At the same time there is a marked decline in craftsmanship and artistic originality. During this period an attempt was made to enlarge and to strengthen the fortifications, to make provisions for the storage of supplies and access to water within the citadels. The period ends with the destruction of all the important strongholds in the Peloponnesus, as a result of which several sites were abandoned and the others were apparently reduced to a shadow of their former power. Pylos was gutted by fire about 1200 B.C. and appears never to have been rebuilt, yet in the Catalogue of Ships Nestor commanded ninety vessels in the Trojan expedition some ten years later. Tiryns, one of the leading cities and the most heavily fortified in the Argolid, had been reduced to rubble and was not reoccupied until later and then only on a very small scale. Prosymna, as we may judge by the contents of the graves, was abandoned. Asine was reduced in size and importance, and many smaller places show signs of having become depopulated after the general destruction at the turn of the century. Such—if we follow the traditional dating—was the condition of the once powerful kingdom of Argos when Diomedes equipped his eighty ships for the invasion into Asia. Mycenae itself, whose ruler led the expedition to Troy with 100 vessels, had suffered a similar fate and at least two of the small industrial towns (Zygouries and Berbati) within Agamemnon's kingdom had been destroyed and very nearly abandoned at the same time.

The disasters which engulfed the Peloponnesus can be dated by the pottery to about 1200 B.C. From the violence and extent of the destruction we may assume that it covered a considerable period of time. At Mycenae, where several houses in the lower city were sacked some time before the end of the 13th century,[16] the citadel may have held out for years against repeated attacks. Some smaller settlements, such as Kourakou and Asine, may have escaped direct assault, but all present a uniform picture of decay and decline in population.

Such, then, was the condition in Greece one or two decades before the Trojan War, the traditional date of which, based on the Parian Marble and Eratosthenes, is 1192–1183. Could a loosely connected alliance of such cities, which only a short time before had been unable to ward off destruction from their heavily fortified citadels, have organized an overseas expedition capable of reducing the kingdom of Priam and its Asiatic allies?

[15] A. J. B. Wace, *Historia* II, 1953, p. 91; *Viking*, 1954, p. 222.

[16] *Historia* II, 1953, p. 92.

Unless we are willing to discount Homer's account of the Trojan War altogether, it is obvious that some means must be found to resolve the discrepancy between the results of the archaeological investigations and the traditional date. Either our dating of Mycenaean antiquities is a hundred years too early, or the dates of the Trojan War and of the Dorian Invasion must be moved back by almost a century. It will not do to explain the expedition against Troy as a ' political and military folly ' made ' at a time when the Greek mainland was threatened from the north '. It may be true that Homer ' makes no attempt to treat it otherwise '[17] than as a folly, but in his story it was the Greeks who won the war. The sorry representatives of a dying culture, who had found precarious shelter among the ruins of their former strongholds, are hardly to be credited with victory in a long drawn out offensive against the eastern power.

By the same traditional reckoning the Dorian Invasion came near the end of the 12th century, approximately 80 years after the Trojan War. The picture presented by the pottery and other archaeological evidence from that period is one of steady decline but not of abrupt destruction. The gradual transition from L.H. III C to sub-Mycenaean is painfully obvious in the steady deterioration of shapes and decorative patterns. Nor was there a general destruction of the cities at this time such as took place about 1200 B.C. The final destruction of some of the houses and of the Granary at Mycenae, which took place in the period of L.H. III C pottery, may have been due to local causes. The general destruction at Mycenae as elsewhere is of earlier date, and there is no evidence for a large scale rebuilding even at Mycenae after 1200 B.C.

So strong is tradition, however, that few scholars have been willing to reject Eratosthenes' dates of the Trojan War and the Return of the Herakleidai. I formerly tried to find a solution of this discrepancy in the mythological account of the dual between Hyllos and Echemos,[18] which seems to imply that the invasion extended over a long period, perhaps as much as a century. This interpretation is not incompatible with the conclusion, more recently reached by Jean Bérard, who has endeavoured to establish a new dating of the events at the end of the Bronze Age.[19] He concludes that the war against Troy took place, not in the beginning of the 12th century but nearly a hundred years earlier (1280–1270 B.C.), and the Return of the Herakleidai near the turn of the century. Fritz Schachermeyer[20] would connect the sack of Troy VII a, about 1200 B.C., not with the war of the Greeks under Agamemnon but with a subsequent attack by the barbarians. Troy VI, on the other hand, which appears to have been destroyed by an earthquake was, according to the same author, the city captured by the Greeks with the help of Poseidon who sent an earthquake (the Wooden Horse) to aid the Greeks in the capture. The last unavailing efforts to make the fortresses of the Peloponnesus capable of withstanding a siege would then fall in the century between the Trojan War and the invasion of Greece by the barbarians. This invasion delivered the *coup de grâce* to a civilization which had exhausted its resources in the Asiatic war and in a costly programme of preparedness and was already in an advanced state of dissolution.

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[17] The quotations are from the late Franklin Daniel, *A.J.A.* LII, 1948, p. 109. This talented scholar, whose untimely death was a great loss to Homeric scholarship, was deeply conscious of the chronological problem involved and sought in this way to reconcile the conflicting records.

[18] *Hesperia* VIII, 1939, p. 426; *A.J.A.* LII, 1948, p. 113.

[19] *Studies presented to D. M. Robinson*, I, pp. 135 ff.

[20] *Poseidon und die Entstehung des griechischen Götterglaubens*, pp. 190 ff. Cf. Martin P. Nilsson's review in *A.J.P.* LXXIV, 1953, p. 167, who expresses a cautious agreement with the author's interpretation of the myth of the wooden horse.

The invasion that profoundly altered the course of history in Greece then becomes part of a larger movement that affected the eastern Mediterranean. For if the coming of the Dorians into Greece is dated about 1200 B.C., it coincides with the collapse of the Hittite Empire and the penetration of the invaders into Syria and Palestine; it precedes by about a decade their arrival in Egypt and their defeat under Rameses III.

At Athens the last defensive measures in the second half of the 13th century may have been effective in saving the city from a fate similar to the sack of the cities in the Peloponnesus. That the threats of war did not vanish without effect upon the city we gather from the abandoned houses on the north-east slope. Whatever the reason, the invaders moved on to more worthwhile conquests in the south of Greece. A later attack, in the reign of Kodros, was also beaten off. The Athenians were permitted to remain in possession of their land, and to become in a peculiar sense the exponents of the culture of Classical Greece.[21]

NOTE.—Reference should be made to the important study of Mycenaean pottery in Athens by Emily D. Townsend, which has just appeared in *Hesperia* XXIV, 1955, pp. 187–219, after the manuscript of the present article had already been submitted for publication. Miss Townsend also emphasizes the independent trends in Athenian pottery, *vis-à-vis* the mainland Mycenaean ware, and points to a closer relationship with pottery from Rhodes and Cyprus, and other Aegean and Eastern sites.

[21] I have had the privilege of discussing the problems raised by this article with Professor Martin P. Nilsson, who kindly read the manuscript and made valuable comments on its contents. For the permission to reproduce the plan, Fig. 1, and three photographs, from the Agora, I am indebted to the Director of the Excavations, Professor Homer A. Thompson. The photograph for PLATE III was kindly supplied by the Editor, O. G. S. Crawford.

# The Camel and the Garamantes

by E. W. Bovill

In A.D. 69 the peace of Tripolitania was broken by war between the neighbouring cities of Oea and Lepcis Magna and the re-emergence of the Garamantes in opposition to Rome. The Garamantes, convinced that if worsted in battle they had only to scuttle back into the desert and fill in the wells behind them to stop pursuit, as they had in the past, and confident in their immunity from attack in Fezzan, had responded to an appeal for aid from the people of Oea and, together with these new allies, they were laying siege to Lepcis. This brought the legate of Numidia, Valerius Festus, hurrying down the coast to restore order. He freed Lepcis, defeated the Garamantes, and pursued them into the desert. In doing so he achieved something remarkable. He found, Pliny[1] tells us, a new and shorter road across the desert, known as the Praeter Caput Saxi. It was probably the road running due south from Oea through Garian, Mizda and El Gheria el Garbia which later on, in the reign of Caracalla, was marked with milestones. The inference is that Valerius Festus discovered a road unknown to the Garamantes. This is incredible for he was operating in the Garamantes' own country which they must have known far better than the Romans and at least as well as any tribe from whom the latter could have obtained guides.

But the mystery surrounding the discovery of the Iter Praeter Caput Saxi extends over a much wider field. Before the end of the century other very odd things happened which have never been explained.

The first was an unexpected *rapprochement* between the Romans and the Garamantes, from which flowed two remarkable Roman expeditions through Fezzan and far into the country beyond. They took place in the reign of Trajan, somewhere about A.D. 100. The first, led by Septimius Flaccus, legate of Numidia, marched for three months southward from Fezzan, which almost certainly must have taken him into the Sudan. Perhaps that country will one day yield evidence of its having done so. Ptolemy, however, thought the story, which he had got from Marinus of Tyre, an exaggeration. The second expedition, led by Julius Maternus, is also recorded by Ptolemy and from the same source. ' Julius Maternus (starting from) the Great Lepcis and marching from Garama in company with the king of the Garamantes towards (against ?) the Aethiopians travelling towards the south, arrived in four months at Agysimba a district or province of the Aethiopians where rhinoceroses congregate '.[2] Ptolemy was reluctant to credit this story also, but it is more circumstantial than the other and rings true to the extent that a strong case has been made for identifying Agysimba with Tibesti.

The association of the Garamantes with the Romans leaves little room for doubt about the object of the joint enterprise. The Romans might of course have endeavoured to cement their new friendship by agreeing to help in an attack on formidable neighbours. There is, however, no evidence of the Garamantes having had any enemies in the south whom they would not have been able to settle with unaided long before the coming of the Romans. It is far more likely that they asked the Romans to go with them on a slave-raiding expedition and that Julius Maternus readily seized this opportunity for

[1] *Natural History* v, 5.

[2] *Geography* I, 8.

seeing new country. That he was himself interested in catching slaves is improbable for the Romans do not appear ever to have concerned themselves with the slave-trade of central Africa. If slave-raiding was the object of the joint enterprise there can be no doubt that Agysimba was Tibesti which is generally accepted as the home of the Troglodyte Ethiopians whom Herodotus tells us the Garamantes used to raid. In the absence of any proof of how far south Septimius Flaccus penetrated, Tibesti may be regarded as the furthest point in the interior of Africa reached by the Romans.[3]

Far more important than these speculations is the problem of what circumstances led to the surprising *rapprochement* and made possible two military expeditions, apparently in quick succession, into remote lands which not long before must have appeared to the Romans hopelessly inaccessible. The strange discovery of the Iter Praeter Caput Saxi is probably part of the same problem. By some odd inadvertance the problem has wholly escaped the attention of the many erudite scholars who have closely studied the history of the Roman occupation. But despite its complexity it is susceptible of a simple though not immediately obvious explanation.

In North Africa the art of war, and much else besides, was at about this time being revolutionized by the camel. In the history of the northern half of the continent no event had greater consequences than the introduction of this now indispensable animal. When it happened and in what circumstances are hedged with much uncertainty. The wide distribution throughout the region of the remains of a quaternary camel for long seemed clearly to indicate that it had been there continuously since long before the dawn of history. It is now known that the quaternary beast did not survive into historical times. The later camel is believed to have been introduced into Egypt by the Persians in the 6th century B.C. Alexander the Great used camel transport on his expedition to the Oracle of Jupiter Ammon, but Siwa appears to have been the most westerly point reached by the camel at this period. When one considers the great need there was for it in Cyrenaica and Tripolitania, let alone the Sahara, this is astonishing. One of the reasons was probably the failure of camels to thrive in Lower Egypt which instead of being, as one might expect, a breeder and exporter of camels was an importer. It still has to satisfy its requirements by buying from Upper Egypt and Palestine. The camel reached the Anglo-Egyptian Sudan some time before the Christian era but its failure to spread from there westwards seems quite unaccountable. Its absence from the numerous early rock drawings which so richly record the desert fauna, both wild and domestic, clearly shows that it did not reach the Sahara.

Caesar captured twenty-two camels at the battle of Thapsus in 46 B.C. This is the earliest record of them west of Siwa. Although the Romans were familiar enough with the use of camels in Asia and can hardly have failed to realize how valuable to them they might sometimes be in Africa they seem to have taken to their use very slowly. Long after Thapsus they were still using horses in military operations to which the camel would have been far better suited. Moreover, in the history of the Roman occupation camels are not again mentioned until 400 years later. In A.D. 363, when the people of Lepcis appealed to the governor of Africa for help against the Austuriani who were ravaging their territory, he replied that he could not come to their aid until they sent him 4,000 camels for transport for his troops.

The Romans may themselves have been responsible for the introduction of camels. It is, however, significant that we first hear of their being used in North Africa not by the

[3] But not, of course, of things Roman. For example in 1931 a Roman coin of the age of Constantine was dug up at Buea in British Cameroons. (Letter from the late E. J. Arnett to the author).

Romans but by those in rebellion against them. Moreover, as their military operations did not normally take them further than the fringes of the Sahara and as nowhere west of Tripolitania did they ever attempt to penetrate the desert their need for them can never have been at all general or pressing.

The introduction of domestic animals is so often associated with the migration of peoples that it is perhaps more than a coincidence that the first appearance of the camel in Roman Africa closely synchronized with the arrival of the Zenata in the same region. These nomadic Berbers, who were destined many centuries later to give the western Maghreb the powerful dynasties of the Merinids of Fez and the Abd el Wadites of Tlemcen, came from the east by way of Cyrene. Whether this migration was inspired or fostered by the Romans we are unable to say, but that the Zenata were responsible for bringing the camel into general use throughout Gaetulia and the steppes of the High Plateaux seems probable.

Somewhere between 46 B.C. and A.D. 363 the Roman army started using camels. It is a very wide margin of time with no data to work on except for the evidence of sculptures showing that in Tripolitania the camel was used for tillage in the second or early 3rd century. Nevertheless, that the Legio III Augusta took to camel transport during the 1st century A.D. seems highly probable, if only because that would explain so much that is obscure[4].

The story of the discovery of the Praeter Caput Saxi road by Valerius Festus immediately becomes understandable. The new road was new only in the sense that it had been too waterless to be used by horses; camels had made it practicable. The 19th century explorer of this very desert, G. F. Lyon, declared that 'horses generally occasion more trouble to a caravan than anything else. The immense quantity of water necessary to be taken for them is always averaged at one camel for each horse, not including other loads of corn or dates for their food'[5]. Not only did the camel make this new road practicable but it made desert travel much faster.

The *rapprochement* between the Romans and the Garamantes can also now be explained. The pursuit of the latter by Valerius Festus and his camelry after their defeat before Lepcis had shown the Garamantes that the Romans could go where they could not and much faster. Gone was their immunity from pursuit, the desert was no more a safe refuge nor could Fezzan any longer be made secure from invasion by filling in the wells. The emergence of this new arm of the Romans had much the same effect on the Garamantes as the atom bomb had on the Japanese in 1945, and with the same result. They at once capitulated and threw their country open to the alien invader.

Finally, we can now see clearly how the great expeditions of Septimus Flaccus and Julius Maternus into the far interior suddenly became possible. The Garamantes had not only lost the will to resist but were courting Roman favours and extending welcoming arms. The camel, too, had made the crossing of the Sahara easier than ever before.

NOTE.—The author is grateful to Mrs Olwen Brogan for helpful suggestions.

[4] Mrs Olwen Brogan has convincingly argued that as the camel was in use by Tripolitanian agriculturists in the second or early third its introduction may well belong to the first century A.D. *Papers of the British School at Rome*, vol. XXII, 1954, p. 126.

[5] *Travels in Northern Africa*, London, 1821, p. 93.

# The Rôle of the Philistines

by Cyrus H. Gordon

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OUR dictionaries tell us that 'Philistine' denotes 'an uncouth or uncultured person'. Archaeologically, this definition is quite unjustified for the Philistines were the main civilizing influence on the Hebrews during the critical period when Israel emerged from rude tribalism to forge a cultured empire. The magnitude of the Philistine impact is reflected in the name of 'Palestine' (=Hebrew *pᵉlɛšeṯ* 'Philistia') which clings to the country after more than three millennia.

The Philistines came to Palestine from Caphtor (Amos IX, 7), whose hub was Crete but which also included many coastal areas around the East Mediterranean, on the mainland as well as the islands. They were products of Minoan civilization and they figure in Egyptian as well as Hebrew historic records.

Hebrew contacts with the Philistines begin at the dawn of Hebrew history[1] in Palestine. Both Abraham and Isaac are said in the Genesis narratives to have had relations with the Philistine King Abimelech of Gerar. The name 'Abimelech' is Semitic showing that the Philistines were already partly Semitized by the time of the Hebrew Patriarchs,[2] which I am inclined to place in the Amarna Age (early 14th century B.C.).[3] Abimelech's general 'Phichol' (Genesis XXI, 22, 32; XXVI, 26), however, still bears a non-Semitic name.

In the Patriarchal Age, the Philistines are depicted as peaceful folk around Gerar and Beersheba.[4] They cannot be regarded as anachronistically retrojected into the Genesis narrative, for the later Philistines in the time of the Judges and early Monarchy were belligerent and inhabited the Pentapolis: Ashdod, Gaza, Ascalon, Gath and Ekron (I Samuel VI, 17). There were many waves of Philistine (and kindred Caphtorian) immigrations to Palestine, spanning most of the 2nd millennium B.C.

The stories of Samson hark back to the days when Philistine pressure was stepped-up and brought to bear on tribal segments of the Hebrew people, notably Samson's tribe

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[1] In referring to the Patriarchal narratives in Genesis as history, I do not mean that the historicity of Abraham, Isaac and Jacob as persons is established fact. Indeed much of the narrative content consists of epic motifs that permeate East Mediterranean literature: Homeric and especially Ugaritic. Yet the setting, the social institutions and ethnic movements are to a great extent historical, and can be used as materials for history by the critical scholar. For example, Abraham is said to have migrated first from Ur of the Chaldees and then from Haran on his way to Canaan. Those two cities were the great centres of Mesopotamian lunar worship. Migrations from Mesopotamian lunar cultic centres are attested independently (notably at Ugarit) by the worship of the Sumero-Akkadian moon-goddess Nikkal in Canaan of the Amarna Age. Accordingly, the Genesis data on Abraham's Mesopotamian residences mean more historically than the origin of one man; they characterize a major source of Canaan's population. The key to utilizing documents like the Genesis narratives is critical evaluation, without which we fall into either naïve gullibility or sterile disbelief.

[2] The Bible gives no hint of any language barrier between Israelite and Philistine in any period.

[3] See my *Introduction to Old Testament Times*, Ventnor (N.J.), 1953.

[4] Cf. Yehezkel Kaufmann, *The Biblical Account of the Conquest of Palestine*, Jerusalem, 1953.

of Dan which eventually had to migrate northward to find a new home. The Philistine menace continued all through the period of the Judges and, in the days of Samuel, forced the Hebrews to organize themselves more effectively under a king. King Saul stood up to the Philistines but his reign ended in disaster in his wars with them around 1000 B.C. Meanwhile, David had received valuable training in the art of war as an ally and protegé of the Philistine King Achish of Gath. After Saul's death in battle, David seized the initiative and, with the help of Philistine warriors[5] (and at first as a vassal of the Philistines), established his throne over Judah and Israel, and then proceeded to carve out the greatest empire in his day—from the Egyptian border to the Euphrates. This accomplishment was made possible by the general local and international situation. The negative factor was the weakness of the nations that had once been, or were later to become, the great powers : Egyptian, Hittite and Mesopotamian. But the chief positive factor was the Hebrew response to the Philistine stimulus.

The Bible preserves enough information about the Philistines for the observant reader to surmise that they were an accomplished people in the arts and crafts, far superior in such matters to their Hebrew contemporaries. I Samuel XIII, 19–23, plainly portrays the Philistines as keeping the Hebrews disarmed through maintaining so strict a monopoly on metal work that there were no smiths in all Israel. Moreover, in making five golden mice as an expiatory gift, they are depicted as competent in the goldsmith's art (I Samuel, VI, 5). Sir Flinders Petrie's excavations in and around the Philistine plain illustrate graphically the artistic output of the area in the second millennium B.C., notably in golden jewellery—contrasting sharply with the general poverty at Judean sites in the fine arts.[6]

It is significant that in Hebrew, *kaftôr* (' Caphtor ') is also used as a common noun to designate a specific decorative device.[7] This word reflects the indebtedness of Israel to Caphtor in the arts. Indeed, so great was the artistic impact of the Caphtorians throughout the entire Levant in the second millennium B.C., that in Ugarit of the Amarna Age, the god of arts and crafts is represented as having his workshop on Caphtor.[8]

Since the language of Minoan Linear B is Greek,[9] it is not surprising that *early* Hebrew vocabulary has loanwords familiar to us from Greek. Hebrew *mᵉḵērā(h)* has long been identified (already in rabbinic literature) as Greek *máḵaira* 'sword', fitting in with the rôle of the Philistines as war lords. The Minoan heritage in architecture[10]

[5] One of his most trusted generals was Ittai of Gath. David used Caphtorian mercenaries called Cherethites (' Cretans ') and Pelethites. It is interesting to note that the Judeans also employed Carian troops (II Kings XI, 4). The people of Crete and Caria were so closely interconnected (Herodotus I, 171) that the ancients may well have reckoned Caria as a part of Greater Caphtor. Caphtor and Caria appear together in a Babylonian text from Hazor (see A. Pohl, *Orientalia*, XIX, 1950, p. 509).

[6] Cf. Flinders Petrie, *Gerar*, London, 1928, pl. I ; *Ancient Gaza* I, pls. XV, and II, pls. I–III, and III, pls. XIV–XV and especially IV, pls. XIII–XX, London, 1931–34, for golden jewellery.

[7] See further my *Homer and Bible : The Origin and Character of East Mediterranean Literature* (in *Hebrew Union College Annual*, XXVI, 1955, pp. 43–108) §40.

[8] The Ugaritic texts are ᶜnt:VI:14 and 51:I:24 ff. The material from Ugarit is available in my *Ugaritic Manual*, Rome, 1955.

[9] For the growing literature on Linear B since the decipherment by M. Ventris (with the collaboration of J. Chadwick), see M. S. Ruipérez, ' Les études sur le linéaire B depuis le déchiffrement de Ventris ', *Minos* III, 2, 1955, pp. 157–67.

[10] This is incidentally reflected in the rôle of the Ugaritic god of arts and crafts, as a Caphtorian builder of palaces.

accounts for the presence of *liškā(h)* ' chamber ' in Hebrew, which cannot well be dissociated from Greek *léskē*. Perhaps the introduction of an improved type of torch accounts for Hebrew *lappîd* ' torch ' (cf. Greek *lampás*, genitive *lampádos*). Hebrew *maṣṣā(h)* usually translated ' unleavened bread ' is verbally related to Greek *máza* often rendered ' barley-bread '. And Hebrew *mûm* ' blemish ' is the same word as Greek *mômos* ' blame '.

The fact that the Mycenaean offshoot of Minoan civilization left its mark on Greece, and the Philistine offshoot of Minoan civilization left its mark on Israel, is of special interest for the origin of western culture. It turns out that early Greek and early Hebrew literatures reflect a common East Mediterranean heritage.[11]

Since the age in question is a heroic age,[12] many details in a discussion such as the present one, hinge on warfare. Thus, in Hebrew the word for a military ' officer ' or ' commander ' is *šâlîš*, derived from numeral *šālôš* ' three '. Homeric texts describing troops, relate that each contingent was organized under three commanders. One such triad consists of Hector and Polydamus, with whom Cebriones serves as the ' third ' (Iliad, XII, 91, cf. 95). Similarly, biblical texts of the Philistine Age have groupings of Hebrew heroes in threes (see II Samuel XXIII and I Chronicles XI). Cf. *Homer and Bible* §95.

Deciding wars by lethal contest between champions (e.g., David and Goliath) appears in the Bible only in the Philistine Age. This too is part of the heroic pattern, familiar from Homer (Iliad, III, 59 ff. ; VII, 74–91). See further *Homer and Bible* §108.

I Samuel XXXI, 8–13 relates that the corpses of Saul and his sons, slain in the Battle of Gilboa, were burned before their bones were buried. Since cremation is out of keeping with normal Semitic usage, and since the battle fits into the period of Philistine domination, the burning of the Hebrew heroes, is a cultural borrowing, probably from the Philistines, certainly from some branch of the Indo-Europeans. It is familiar from Greek epic ; witness the burnings of Patroclus or Hector. It characterizes the whole gamut of Indo-European epic, all the way from India (Mahabharata) to England (Beowulf and the Fight at Finnsburg). Its absence from the Gilgamesh Epic confirms the Indo-European character of heroic cremation.[13]

As depicted in Egyptian representations of the Keftiu (=Caphtorians), so too the hair of the Greek (e.g., Homeric ' long-haired Achaeans ') and Hebrew (e.g., Samson's seven locks of hair) warriors was long in accordance with one and the same Caphtorian style (*Homer and Bible* §98). Cf. the long curls modelled on the anthropomorphic sarcophagus cover excavated by Flinders Petrie (*Beth-Pelet* I, London, 1930, pl. XXIV ; upper left corner).

So closely interrelated are early Hebrew and early Greek cultures (due in large measure to the East Mediterranean common denominator), that problems in the history of the one people become clarified in the light of facts in the history of the other. David, the singing poet, fits into the East Mediterranean picture we have for heroes, in many ways ; for example, Achilles and Paris are *incidentally* portrayed by Homer as skilled in music and song. But the *heavy emphasis* on David's musical accomplishments would

[11] In broad outline this has been clear for centuries. However, meticulous scholars have tended to shy away from such comparative considerations because of gaps in our knowledge. But recent discoveries such as those at Ugarit and the decipherment of Linear B are bridging the gaps to such an extent that continued scepticism is no longer tenable.

[12] For the term, cf. H. M. Chadwick, *The Heroic Age*, Cambridge, 1912.

[13] For the ancient literature of Western Asia and Egypt, see J. B. Pritchard (editor), *Ancient Near Eastern Texts*, 2nd ed., Princeton, 1955.

remain a mystery since his great historic rôle was of a decidedly military character. He shook off the yoke of Philistine domination and then conquered a large empire in Canaan and Aram. Through military means he raised Israel from defeat and subjugation to victory and empire. Without collateral information we could hardly explain the stress laid on his musical ability. A clue to the mystery is the fact that, while the texts never attribute the use of cavalry or chariotry[14] to David's armies (see, for example, I Chronicles XII), they do state that he or his generals vanquished Aramean cavalry and chariotry; cf. II Samuel VIII, 3–4; X, 18; I Chronicles, XVIII, 3–4; XIX, 6, 18. Infantry, trained to function in close order, could be more than a match for cavalry and chariotry, not only in the hills of Judah or Ephraim (where the terrain favours the use of infantry) but even in more level areas. But to train such troops and lead them to victory in battle, the leader must be one who can keep the men in step and integrate the individual in all his motions into the army as a harmonious team. Under such conditions, the leader can put to good use his talents as musician, singer and poet. That David put his muse to work for military training is indicated by II Samuel I, 18 where the function of a poem of David is 'to teach the sons of Judah the bow'.[15] There is a close parallel in Sparta (whose institutional affinities to Crete are very close[16]): Tyrtaeus, through his poetic compositions and marching songs, incorporated the individual Spartan soldier into the tactical unit. The musician can be the best general for those who march and fight in close order. The system built by Tyrtaeus put an end to individual combat and the need for horsemanship.[17]

The national epic of the Hebrews is the Exodus, which shares much in common with Philistine traditions. Amos IX, 7, plainly states that the Exodus of Israel from Egypt is not unique but on the contrary is of a piece with the Philistine exodus from Caphtor and the Aramean exodus from Kir. In Amos's day, all three exoduses were apparently current in oral epic form. Fathers transmitted them to their sons. Shepherds and wandering poets like Amos would meet other shepherds and wandering poets of other local nations and exchange their epic traditions. Thus not only is Amos familiar with the Philistine exodus, but I Samuel IV, 8, represents the Philistines as conversant with the Israelite exodus.

As we have already noted, the word for the 'unleavened bread', *maṣṣā(h)*, of the Exodus is the same as Greek *mâza*. In connection with the East Mediterranean character of the word it is interesting to note that the Hebrew epic emphasis on the bread baked for a vast multitude about to march, is significantly paralleled in a Ugaritic epic of the Amarna Age. For in the Epic of Kret,[18] the hero (who bears the Cretan name of Kret) is represented as baking a huge supply of bread for his army on the eve of its departure on a distant mission.

The Caphtorian link between Canaan and Hellas in the wake of the Amarna Age puts a new light on many familiar episodes in early Greek and Hebrew classics. In the

[14] In David's reign, the horse-drawn chariot occurs as a sign of high rank (II Samuel XV, 1) rather than as a branch of the armed forces. By Solomon's reign, however, horses and chariots became important in Israel's military power. Solomon's stables excavated at Megiddo confirm the Biblical account (I Kings, V, 6; IX, 19, 22; X, 26).

[15] This poem of David (like others of Tyrtaeus) is elegiac. That compositions designed for military training should be of mournful character serves to remind us that social psychology in the ancient Near East differed considerably from ours.

[16] Herodotus, I, 65.

[17] See E. Meyer, *Geschichte des Altertums*, 2nd ed., III, Stuttgart, 1937, p. 515.

[18] The text is Krt: 83–4, 174–5.

Book of Judges (xv, 3–8), Samson, enraged because his Philistine wife is taken away from him, wreaks a dire revenge on the Philistines so that many of their number perish (verse 8) before his wrath is assuaged (verse 7). However simple and folkloristic the 'wrath of Samson' may be, it is genetically related to the more famous 'wrath of Achilles', on account of which many brave Achaeans perished before it was appeased. Nor can we mistake the Caphtorian milieus of both wraths—one Philistine, the other Mycenaean (cf. *Homer and Bible*, §92).

In a heroic age, dislocation and uncertainty are the order of the day. Petty warfare and raids were frequent; women and children were often carried away. David's wives and children were carried off by Amalekite raiders at Ziklag. He recaptured them. Menelaus lost Helen and Achilles lost Briseis; both heroes recovered their belles. The theme was so popular that the public could not get enough of it. Twice the Philistine King Abimelech cast covetous eyes on the wives of the Patriarchs; once on Sarah, once on Rebecca. He actually took Sarah into his harem. Pharaoh also took Sarah into his harem. But all three episodes ended happily with the heroine in the arms of her rightful husband. This feature became an integral part of East Mediterranean royal epic. Ugaritic epic tells how King Kret recovered his beloved Ḫurray and the Bible tells how King David recovered his wife Michal, even as Greek epic tells how King Menelaus won back Helen (*Homer and Bible* §89). Indic epic shows that this was an Indo-European contribution to the East Mediterranean.[19] The earlier literatures of Mesopotamia and Egypt show no romantic interest in women or marriage.

We are traditionally raised on the concept that the three main fountainheads of western civilization are Israel, Greece and Rome. This is an oversimplification because of the debt each of these owed to its component sources. The central source that most affected all three was the East Mediterranean culture that we may term either Minoan or Caphtorian. Insofar as the Philistines were the Caphtorians who enriched Hebrew culture so abundantly in the arts, they ought to be recognized as a major source of a fountainhead of occidental civilization.[20]

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[19] The Mahabharata and Ramayana narratives are digested and explained in M. Winternitz, *A History of Indian Literature* I, Calcutta, 1927, and N. K. Sidhanta, *The Heroic Age of India: A Comparative Study*, London and New York, 1929.

[20] The bibliography on the Philistines is too extensive for repetition in an article like this one. But there is one outstanding book that should not be passed over in silence; to wit, R. A. S. Macalister, *The Philistines: Their History and Civilization*, London, 1914. From the varied new data bearing on the problem, it might be worth singling out the fact that navigation between Ugarit and *mât kaptu-ri* 'the land of Caphtor' is now attested in an Akkadian tablet from Ugarit; see J. Nougayrol, *Le palais royal d'Ugarit* III, Paris, 1955, p. 107 and pl. LXIX, text 16.238:10.

# The Sword of Korisios*

by René Wyss

*Bern Historical Museum*

During the reorganization of the prehistoric section of the Bern Historical Museum a chance discovery was made by the writer of the present article. While a fragment of a La Tène sword (without inventory number) was being treated, it was suspected that a punch-mark might be concealed beneath the scabbard, which was rusted on to the sword. The rusty remains were removed, so that the blade could be examined in an oblique light. Most surprisingly there was revealed not only what subsequently proved to be a well preserved punch-mark but also, beside it, a Greek inscription! After taking a photograph of it in its original state the remains of the scabbard were removed, and a single letter of the inscription that was covered by rust was cleaned (PLATE VIII, B, C).

In order to discover the provenance of this extraordinary find all the Museum's La Tène swords were examined with respect to their typology and find-spots. By a remarkable piece of good fortune the lower part of the blade belonging to our fragment was recovered (Inventory number 19035). According to our catalogue this well-preserved sword-blade was found, during the rectification of the Jura waterways (1868–75) in the Nidau-Büren canal, and this is confirmed by a published drawing and description. As will be seen the sword was found, unless we are much mistaken, at Port. When found it was bent almost at a right angle, as the broken edges showed. Unfortunately it was forcibly straightened, probably by the finder, who was probably not aware of the special significance of bent swords, and thus the blade was broken in half.

The overall length of the sword is 96 cm. and its maximum width 5.4 cm., ending in a short point. It has a sharply defined midrib ending in a flat-hammered tang. Parallel to the midrib run two wide grooves (Blutrinnen), with narrow grooves on each side. As often happens, the edges have been welded on to the sword, which was probably made with the usual damascening technique. It is a cutting weapon of a Late La Tène type, representing considerable technical accomplishment. Below the guard can be seen an oval punch-mark which has been stamped into the red-hot iron with such force that the outline is faintly visible even on the other side of the blade. Stamped below the punch-mark can be seen the word KORISIOS, written in Greek letters. The punch-mark shows a palm-tree symmetrically flanked by two horned animals rampant, probably capricorns, eating from the tree. This heraldic design can be traced back to an oriental prototype, and was probably derived from an ancient classical intaglio; whereas the commoner punch-marks, mostly of star-signs or animals such as bulls or boars, seem to be derived from Celtic, or perhaps even Greek, coins.

It is well known that Celtic swords represent a unique achievement and a high point in the old art of sword-making, and the employment of a punch-mark must be regarded in the light of this fact. No doubt a famous sword-smith enjoyed a high repute amongst the warlike Celtic peoples, who liked to be able to distinguish his products from those having no punch-marks. This is clear from the fact that punch-marked swords

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* Translated by Dr Maria Bersu and the Editor. The article was first published in *Ur-Schweiz* XVIII, 1954, 53–7; we wish to thank the Editor, Prof. Dr Laur-Belart, for permission to republish. Another account is printed also in the *Jahrbuch des Bernischen Museums in Bern* XXXIV, 1954, 201–22.

are always above the average in quality. The maker of our inscribed sword must surely have been one of the outstanding Celtic armourers of the region, as the selection of so artistic a punch-mark shows ; that is shown even more clearly by the addition of his name in Greek letters—always provided that we are correct in interpreting the punch-marks as protective symbols or trade-marks, rather than as owners' marks. The choice of motives for the punch-marks is unlikely to have been fortuitous. We may suppose that the horns of these two capricorns were thought to give a protection against black magic ; and that their position beside the Tree of Life, was a magic symbol of defence and also of the life-power of the owner of the weapon.

If that conjectural interpretation is correct, the punch-mark on another sword from Port, showing a cow or bull lying on the ground, assumes a special significance (PLATE VIII, A). A water course flowing through the Lakeland is said to have been called Kuhfluss (Cow River) in Celtic times; and a ring from Port, already published several times, has a design of bull's horns and waterfowl. These facts suggest a bull cult in the region. Perhaps the symbol of the river-god as a bull stamped on the sword endowed the owner, by means of a magical process, with special power.

The mental attitude revealed by the symbols may help to explain the dedication of the sword to the dead warrior after it had been made valueless by bending. The ritual custom of bending swords, and even of folding them up several times, caused Polybius and Plutarch to assert that these swords were useless even after the first cut unless the warrior could unbend them quickly.

The sword of Korisios is one of these ritually bent blades (with remains of the scabbard!); it seems to have come, not from a grave but from the bed of an old river, and we may suppose that it was an offering to the river-god. In this connection it is important to mention the latest interpretation of the famous site of La Tène itself, at the lower end of Lake Neuchâtel, and of Port, at the lower end of Lake Biel, as being Celtic places of sacrificial offerings. Though both sites have yielded remains of piles, there are no remains of habitations. On the other hand there are many finds, mostly of weapons—from La Tène no less than 166 swords (mostly in their scabbards) and 270 spear-heads. Most of the swords from La Tène belong to the Middle La Tène period, whereas most of those from Port belong to La Tène III and include a type which seems to be characteristic of Port, namely, that to which the Korisios sword belongs. Our inscribed sword is probably, therefore, if we accept K. L. Raddatz's convincing interpretation, one of the weapon-offerings discovered in an old river-bed during the rectification of the 19th century. The large number of swords, probably offerings to a bull-shaped river-god, are many of them of the highest quality, which may at first thought, appear surprising. But we know that the Celtic peoples were fond of making offerings, and that they did occasionally make human sacrifices ; so it may not be without significance that, amongst the hundreds of weapons found at La Tène, some human skeletons and skulls also have turned up. It should be recorded, too, that an old report speaks of the finding of a skeleton with a noose of hemp round its neck—an occurrence hitherto recorded only in Northern Europe.

NOTE.—Dr Wyss cites examples of the name Corisius occurring in a dedication inscription (*CIL*, IX, no. 2828), Corisia as a girl's name on a tombstone (*CIL* V, no. 7184), and Corisio as a potter's name (*CIL*, VII, no. 10010/646). Corio- is a Celtic word meaning war host, war assembly. We might add that the word is found in some Romano-British place-names in the north, and that it may have been an element in the certainly corrupt name of Corbridge, Corstopitum ; an emendation to Corioritum or Corisioritum is here suggested, where the second element is the Celtic word for ' ford.'—EDITOR.

# Books Received*

*The inclusion of a book in this list does not preclude its subsequent review*

Rome Beyond the Imperial Frontiers, by Sir Mortimer Wheeler. Penguin Books. 3*s* 6d. [The Penguin edition of the book reviewed in Antiquity xxix, 1955, 48–9].

The Micmac Indians of Eastern Canada, by Wilson D. Wallis and Ruth S. Wallis. Univ. of Minnesota Press, Minneapolis. (London: G. Cumberlege). £3.

Basket Maker ii Sites near Durango, Colorado, by Earl H. Morris and Robert F. Burgh. Carnegie Institution of Washington, D.C., 1954. $4.25.

Archaeological Excavations in Mesa Verde National Park, Colorado, 1950, by various authors. Nat. Park Service, Washington, 1954.

The Kaska Indians: an ethnographic reconstruction, by John J. Honigmann. Yale Univ. Press, 1954. $2.00.

Proceedings of the 30th International Congress of Americanists, held at Cambridge, 18–23 August 1952. Published by the R. Anthropological Institute, 21 Bedford Square, London, W.C.1. [We regret inability to review this excellently produced book of 250 pages (no price stated)].

The Mildenhall Treasure: a handbook, 2nd edition, 1955. British Museum. 2*s*.

Early Medieval Art, by Ernst Kitzinger, 2nd edition, 1955. British Museum. Price not stated, but ascertained to be 7*s*.

Kungl. Vitterhets Historie och Antikvitets Akademiens Handlingar: Antikvariska Serien i. Arpad Weixlgärtner, Das Reliquiar mit der Krone im Staatlichen Historischen Museum zu Stockholm. Stockholm, 1954. [A fully illustrated account of the important medieval reliquary sent to Stockholm in 1631 from the Swedish H.Q. in Germany. The original setting of the agate cup dates from *c.* 1000; the transformation into a reliquary with the added foot and cover was carried out in the middle of the 13th century. The setting includes classical and later gems].

Kungl. Vitterhets Historie och Antikvitets Akademiens Handlingar, Part 83: Antikvariska Studier, V. Stockholm, 1953. [The articles include an account of the older megalithic pottery of Sweden, a publication of the coin hoard from Sigsarve, Gothland, buried *circa* 1055–60 and including many Anglo-Saxon coins, and a list of mason's marks in the Cathedral of Lund].

Kungl. Vitterhets Historie och Antikvitets Akademiens Handlingar, part 89: Bankälla och Stora Ro by K. E. Sahlström and N-G. Gejvall. Stockholm, 1954. [A detailed report on the excavation of two cemeteries in Westergotland—about 70 miles ne of Gothenburg. The few objects found indicate a continuous use of the cemeteries between about 100 B.C. and A.D. 100. German summary].

Acta Instituti Romani Regni Sueciae, octavo series 8: De Pallio und die stilistische Entwicklung Tertullians by Gösta Säflund. Lund, 1955. [Contains a text with a full discussion of the date of the work and its place in the development of the author's style].

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* We regret the length of this list, caused partly by lack of space for reviews, partly by the difficulty of finding reviewers willing to act. Ed.

KUNGL. VITTERHETS HISTORIE OCH ANTIKVITETS AKADEMIENS HANDLINGAR, part 70, 2: Tauschierte Metallarbeiten des Nordens aus Römerzeit und Völkerwanderung, by WILHELM HOLMQUIST. Stockholm, 1951. [A comprehensive survey of the Germanic inlaid metal work of the Roman age and the Migration period. The provincial Roman background is also covered. The English material has been more fully listed and discussed in the later article by Miss V. I. Evison in *Antiquaries Journal*, XXXV, 20–45].

DOWN TO EARTH, by ROBIN PLACE. London: Rockcliffe, 1955. 16*s*. [A useful introduction to the study of British prehistory, not intended for the specialist. Compression has led to a number of misleading statements and some errors].

THE HOLY WELLS OF WALES. By FRANCIS JONES. $8\frac{1}{4} \times 5\frac{1}{4}$. pp. XXII+226 with six maps. Cardiff, University of Wales Press, 1954. 12*s* 6*d*.

This book is in two parts. The second is a list of the holy wells recorded in Wales, county by county, showing the association of each with its saint or church, its reputation for healing or such other distinction as is known. The first part is in the nature of a commentary on the list. The author regards the cult of wells in Wales as of ancient pagan origin taken over and modified by the medieval church and surviving, in spite of the hostility of the reformers, down to the present day. This argument is presented so much in the form of a catalogue that its validity is difficult to test. Too much of the information is modern and of doubtful interpretation, and too many of the medieval references are cited from secondary sources.

C.A.R.R.

INDUSTRIE, 9e année, No. 10, October 1955.

This issue of *Industrie* is devoted to Belgian road problems, and with commendable logic includes an article on the Roman road system of the country by Joseph Mertens. M. Mertens has contrived to compress into a few pages an admirable account of the purpose and historical importance of the roads, the methods of tracing them, some details of their construction (with illustrations) and an account of the course of twenty-three of them, with bibliographical notes. Not quite so admirable is the appended map (the very odd nexus west of Bastogne does not agree with the description in the text, nor does the course of Route 16 near Dinant); but this is only a sketch, and one may expect that it will be replaced by something better in the larger work which is in preparation. In the meantime one can, with Mertens and Michelin, supplemented by de Maeyer, get a fair idea of the outlines, if not of Gallia Belgica, at least of Roman Belgium. One can also reflect on the fact that *Industrie* is published by la Fédération des Industries Belges.

A. L. F. RIVET.

THE SARAWAK MUSEUM JOURNAL, Vol. VI (No. 5, New Series: No. 20, Old Series), July, 1955.

RUNA: Archivo para las ciencias del hombre, Vol. VI, parts 1–2, 1953–4 (received November, 1955).

JAHRBUCH DES R-G. ZENTRAL MUSEUMS, MAINZ, 2 Jahrgang, 1955.

TOFTING, eine vorgeschichtliche Warft an der Eidermündung, by A. BANTELMANN. Wachholtz, Neumünster, 1955. DM. 36.

MONASTIC BRITAIN, Map of (North Sheet), 2nd edition. Ordnance Survey Office, Chessington, Surrey. 9*s*.

THE TEMPLE OF NEHALENNIA AT DOMBURG, by ADA HONDIUS-CRONE. Meulenhoff, Amsterdam, 1955.

THE DOWNPATRICK GOLD FIND, by V. BRUCE PROUDFOOT. H.M.S.O., Belfast, 1955. 5*s*.

BONAM PAK, CHIAPAS, MEXICO, by various authors. Carnegie Institution, Washington, 1955. $3.00.

DER AUFSTIEG DER MENSCHHEIT, by DR H. KÜHN. Fischer Bücherei, Frankfurt on Maine and Hamburg. [A sort of German Penguin, price not stated].

THE INHERITORS, by WILLIAM GOLDING. Faber & Faber. 12*s* 6*d*.

LES HOMMES DE LA PRÉHISTOIRE, by A. LEROI-GOURHAN. Editions Bounelier, Paris. Price not stated.

THE PHONOLOGY AND MORPHOLOGY OF ROYAL ACHAEMENID ELAMITE, by HERBERT H. PAPER. Univ. of Michigan Press. (Cumberlege. £1 12*s*). [We fear that this notice is yet another example of the neglect of the Elamite language. Here is a grammar of it].

RADIOCARBON DATING, by W. F. LIBBY. 2nd edition, 1955. Univ. of Chicago Press. Cumberlege. £1 14*s*.). [Gives an up-to-date list of dates ascertained, and is thus indispensable, apart from its other contents].

BURIED LONDON : Mithras to the Middle Ages, by W. T. HILL. Phoenix House, 1955. £1 5*s*.

CUPID AND PSYCHE, by JAN-ÖJVIND SWAHN. Gleerup, Lund, 1955.

GREEK COINS by CHARLES SELTMAN. Methuen, London, 1955. XXVI+312 pp., 9 text figures, 4 maps and 64 plates. Price, £2 10*s*. [This is the revised second edition of what is still the only comprehensive historical account of Greek coinage. The author is an expert, and like many experts has initiated many heresies. His tenacity of them in the face of detailed criticism detracts a little, but only a little, from the value of the book. The illustrations are excellent, both in number and quality.—JOHN BOARDMAN].

ATHENA PARTHENOS AND ATHENA POLIAS, by C. J. HERINGTON. Manchester University Press, 1955. 70 pp. and a plan. Price, 10*s*. 6*d*. [A singularly readable account of the worship of Athena on the Acropolis at Athens, and of the Athenians' attitude to her as goddess of the city and maiden goddess of war. The archaeological evidence is somewhat over-simplified, but the general approach is reassuring, and deductions sound. Herington's explanation of the point of the Parthenon is not new, but it deserves the clear exposition is receives here.—JOHN BOARDMAN].

# Important New Books and Articles

*The inclusion of a book in this list does not preclude its subsequent review*

A Paint Mine from the Early Upper Palaeolithic Age near Lovas (Hungary Province of Veszprém). *Acta Archaeologica Acad. Sci. Hung.*, Tom v, 1954, fasc. 1–2, pages 1–32: written in English, with Russian summary. [A careful and detailed description of an interesting discovery. We welcome the use of the English language to record it].

Geologic–Climatic Dating in the West, by Ernst Antevs. *American Antiquity*, vol. xx (April, 1955), 317–35. [An authoritative and fully documented statement on the present position: correlation of American and European climatic phases, taking account of the most recent results from radiocarbon and other dating techniques].

Excavations and Experiments in ancient Irish cooking-places, by Michael J. O'Kelly. *Journ. R. Soc. Antiq. Ireland*, vol. lxxxiv, 1954, 105–55. [A valuable and conclusive account of a widespread type of settlement].

Aspects of the Ascent of a Civilization, by Sir Mortimer Wheeler. Hobhouse Lecture No. 25, Cumberlege, 2*s* 6*d*, 1955. [An optimistic interpretation based on first-hand knowledge].

Les Eyzies: a Test Excavation, by Hallam L. Movius, Jr. *Archaeology* vii, No. 2 (Summer, 1954), 82–90.

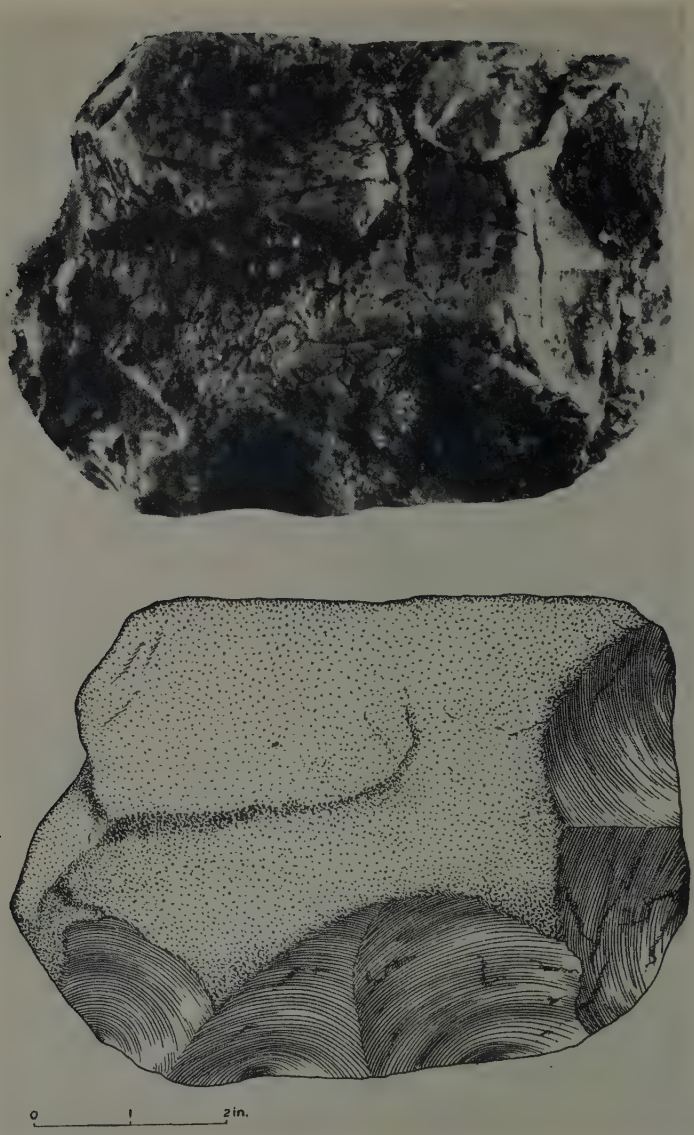
Onomastica Iberica e matriarchato mediterraneo, by the late Vittorio Bertoldi. *Revista Portuguesa de Filologia*, ii, 1948. Coimbra: Casa do Castelo. [From Latin inscriptions in Aquitania he infers gods associated with animals, and translates the words by means of their Basque equivalents, which are related to cognate words in Berber. These Basque words are found in modern Iberian names, e.g. Velasquez, containing Basque *beltx*, black. aherbelste deo=to the Black Ram God (Basque *ahar*=Berber *akâr*). These were also Black Bear and Black Donkey Gods].

Indo-European Languages and Archaeology, by Hugh Hencken. *Amer. Anthr. Assocn.*, vol. 57, No. 6, Part 3. Mem. No. 84. [An important survey of the problem which we hope to review later].

# PLATE I

RESTORATION OF SPECIES OF *AUSTRALOPITHECUS* FOUND AT STERKFONTEIN IN THE TRANSVAAL
Drawn by Maurice Wilson
(see pages 5 ff.)

*facing p.* 32



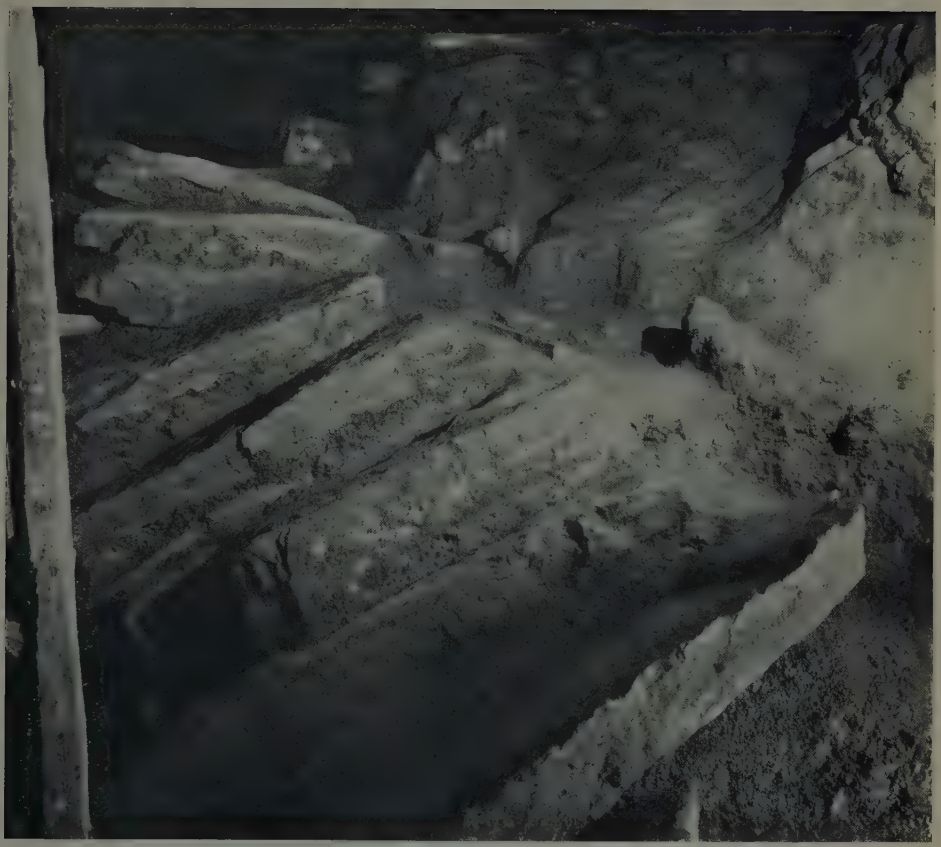

ONE OF THE SUPPOSED PEBBLE-TOOLS IN DOLOMITE FROM GRAVEL CONTAINING REMAINS OF *AUSTRALOPITHECUS* AT THE MAKAPAN LIMEWORKS, TRANSVAAL

*Above:* photograph by Revil Mason
*Below:* interpretative drawing by Professor C. van Riet Lowe
*By courtesy of the Director of the Archaeological Survey of South Africa*

MYCENAEAN WALL ROUND THE ACROPOLIS OF ATHENS BEYOND IS THE PARTHENON
(see p. 10)

THE PATH LEADING UP TO THE POSTERN GATE IN THE MYCENAEAN WALL OF THE ACROPOLIS, ATHENS; IN FOREGROUND, THE FLOOR OF A LATE MYCENAEAN HOUSE

(see p. 12)



COLLAPSED STAIRWAY IN FOUNTAIN SHAFT, ACROPOLIS OF ATHENS

(see p. 13)

CARVED IVORY BOX, FOUND IN 1939 IN A CHAMBER TOMB BELOW THE AREOPAGUS, ATHENS
(see p. 14)

*a*. VASES FROM CHAMBER TOMB BENEATH THE TEMPLE OF ARES

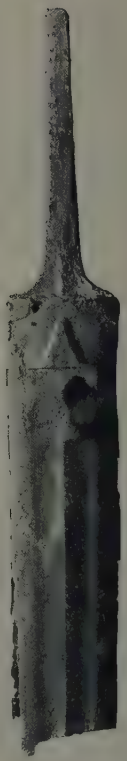
(see p. 14)

*b*. SUB-MYCENAEAN VASES FROM A GRAVE IN THE AGORA, ATHENS

(see p. 15)

A

B



C

*a.* PUNCH-MARK ON A SWORD FROM PORT, SHOWING A COW OR BULL LYING DOWN
*b.* NAME AND PUNCH-MARK ON THE KORISIOS SWORD, ALSO FROM PORT
*c.* UPPER PART OF THE KORISIOS SWORD

# Correspondence

## THE INSCRIPTION OF BAHA IN KAFA. (ANTIQUITY, Sept. 1955, p. 164).

My friend, Dr Ullendorff's version of Cecchi's copy of the Baha inscription is, of course, better Ethiopic than mine, and as an *Ethiopic* text is much more satisfactory, especially the last line, which completely defeated Cecchi. But I have long had a suspicion that this inscription, though in Ethiopic characters and words, was written by one to whom Ethiopic was unfamiliar, possibly a converted pagan. In Kafa the struggle between the dominant spirit-cult and Christianity was a very definite historical fact, and the conquest of paganism by Christianity (as it may have seemed at the time[1]) might well have been recorded in the last line of the inscription, as I ventured to suggest in my interpretation: 'the giver of the *tābot* built a church, and destroyed the *germā* of paganism'. Though this may not be an Ethiopic mode of expression, it could on the other hand render in the mind of a Kafa an expression like the Kafa sentence *bārē qēto haggite, eqqē womā dubite*, 'he built a church, he brought to nought the power of the *eqqo*'. It is also important to realize that in Kafa, never part of Ethiopia till 1897, Sartsa Dengel, was not *germāwi*, for in Kafa the only beings that might be referred to as 'His Majesty' were Dochcho Atiochcho the king of the *eqqo*, and his earthly representative the Kafa king. Such a reference to an outsider, even were he the king of Ethiopia, would hardly have been tolerated in Kafa, whether in a Christian church or elsewhere; and for this reason I took the last word *ger* . . . as *germā*=Kafa *womā*, power, rather than as *germāwi* referring to Sartsa Dengel. It must also be remembered that we are dealing with a faulty copy of an incomplete text, with gaps the length of which we do not know; this in itself allows some latitude in interpretation in the peculiar circumstances of this case; and in a strongly pagan area like Kafa, outside the Ethiopic sphere, too much emphasis should not be laid on purely linguistic aspects to the exclusion of the ethnological aspect. In the last line of the text the second word, *sahurtsa*, can hardly represent *sartsa*; my emendation, whatever else may be said of it, has at least the same number of letters (four—*waharatsa*) as Cecchi's copy, which *sartsa* (three letters) has not. In line 4 it is to be noted that Cecchi's copy indicates no omission after *amlāk*, though it does after *Māryām* in line 3. In line 1 I must admit that Cecchi's translation 'degli altari' misled me into altering *ze* to *za* (also my *za* for *ze* in line 4), and Dr Ullendorff is undoubtedly right in giving 'This is the altar'. I should like to correct an accidental omission in line 2 of my transcription; *tābot* should of course be *tābota* (as in Cecchi's copy), and the corresponding translation should be 'altar of St. George'.

G. W. B. HUNTINGFORD.

* * * *

A passage from my article on the Third Pan-African Congress on Prehistory reads as follows (ANTIQUITY No. 116, p. 207): 'From the graphic expression of the behaviour of the primary classes, Mason has been able to distinguish thirteen separate industries in the M.S.A. of the Transvaal'. Mr Revil Mason has since pointed out that his statistical scheme is *based* on evidence provided by thirteen stratigraphically distinct assemblages and it is this evidence which is reflected in graphic expression, not the other way round. I must apologize to Mr Mason and to readers of ANTIQUITY for my misinterpretation of this point.

SONIA COLE.

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[1] See the summary of the history of Christianity in Kafa in my 'Galla of Ethiopia', *Ethnographic Survey of Africa*, 1955, p. 133.

# Notes and News

## THE BLUE-STONES OF THE CARDIGAN DISTRICT

While investigating the geological history of the Teifi valley, I discovered a large stone near the town of Cardigan which had once been used as a gate post. The stone is of a type of rock represented by the majority of the Blue-stones of Stonehenge. Dr H. H. Thomas demonstrated many years ago that these were similar to rocks which occur at Carn Meini on the crest of the Preselly range in North Pembrokeshire. The conclusion that these stones had been transported from Pembrokeshire was confirmed by the fact that a few other Blue-stones of very different types and geological history occur together on the mountain within a short distance from Carn Meini. It is impossible to explain their transport except by human agency.

The Carn Meini rock crops out on the crest of the range about three miles from the Eastern end and about four miles east of the summit point (1760 ft.). On a fresh surface the rock is dark green with numerous rounded spots of felspar ranging from one-eighth to one-half of an inch in diameter and occasionally even larger. Some specimens are more ' spotted ' than others, and occasional examples may be found in which spots are absent. The rock weathers brown with a fairly rough surface and the stones have usually one or two flattish surfaces indicative of the columnar structure of most of the rock.

Stones of this kind are strewn abundantly on the surface of the mountain near Carn Meini and it is not necessary to quarry the rock to obtain them.

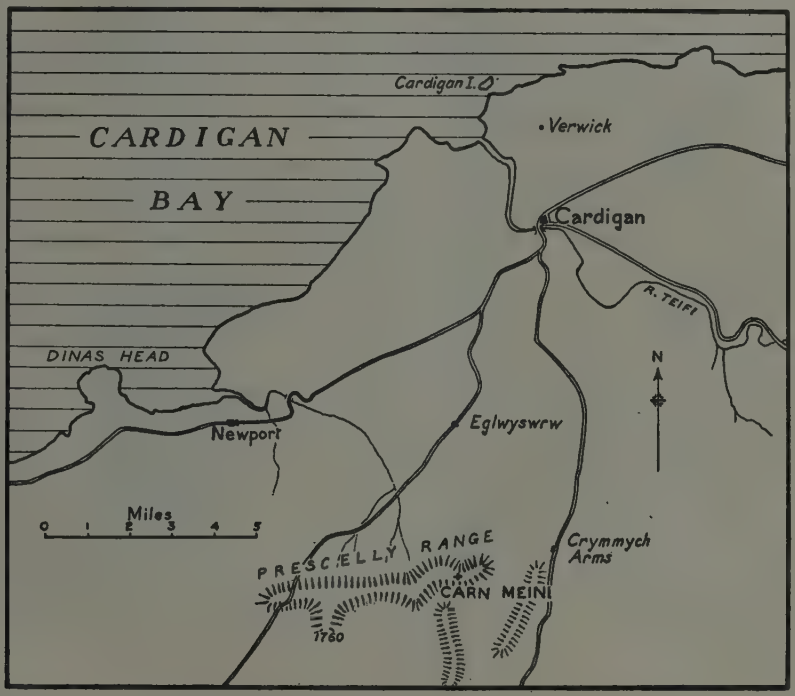
In the Cardigan district there are innumerable large boulders which were carried into the area during the Ice Age from northern regions, most from North Wales, but some even from the South of Scotland. Since Cardigan lies to the north of Carn Meini it is obvious that ice transport cannot account for the presence of this type of rock near Cardigan. As the stone appeared to be of sufficient interest to be preserved, I reported the discovery to the Editor of the local newspaper. The editor communicated with Alderman Aneirin James, who had formerly owned the land on which the stone occurs. The result was surprising. Within a few days Alderman and Mrs James discovered several more stones of the same type on their own farm and later several more on adjoining farms which appear formerly to have been parts of the same estate. In the meantime I had discovered another bringing the total number to 22.

In every case the stones have been erected as gateposts for which their massive size and columnar jointing (which gives them certain flat faces) makes them very suitable. It must, however, have been difficult to bore the necessary holes in rocks of such size and remarkable toughness

Their present distribution, so far as it has been investigated, seems to be related to a condition of the countryside which has long since ceased to exist. In olden times the road from Cardigan to the hamlet of Verwick, three miles to the north, crossed a stream near the north end of the town and passed to the south of Tre-bared farm. More recently a new road was built from Cardigan to Gwbert, at the mouth of the Teifi estuary and parts of the old road in consequence ceased to be used, though it can still be traced. Three of the Carn Meini stones occur as gate-posts on the eastern side only of this old road south of the Gwbert road, and eight more on the eastern side, north of that road. Seven others are found straddling a secondary lane leading to Stepside farm ; one of the stones which gave access to this lane from the old road carries a Bench mark 180·3, while the tallest

example in the area, now whitewashed, occurs at the entrance to the farm from the Gwbert road.

The stone that first attracted my attention is about 170 yards down this road from the farm entrance and about 20 yards to the west of it. It stands about 5 feet 6 inches out of the ground, is over 2 feet wide and in places more than 1 foot thick, and must weigh altogether more than a ton. Near the road is a pair of gate posts made of the local slate-rock formerly quarried at Cilgerran, three miles away. These and the single




Carn Meini stone obviously were gate posts on a road of which there is now no other indication that it ever existed. In continuation of its trend and on the other side of the Gwbert road is the lane that leads to Tre-Gibby farm and on it are three other Carn Meini stones. Thus nine of the stones lie on the approach road from the old road to Stepside farm and five more on what was probably another road giving access from it to Tre-Gibby. So far as is known therefore all the stones are on ancient roads.

The erection of the stones as gate posts probably occurred in connection with active land inclosure of the area. It is, however, difficult to believe that these great stones, each weighing between 13 and 23 cwts., at least, were hauled all the way from Carn Meini to the Cardigan district merely to serve as gate posts, especially as it is probable that the Cilgerran slate-quarries were working and could have supplied more tractable posts of sawn slate.

The reasons for the transport of the Blue-stones from Carn Meini to Stonehenge is, of course, a complete mystery. One possibility is that their columnar form may have been widely known to Bronze Age man and they were therefore regarded as desirable material by the architects of those days. Or, again perhaps by reason of their shape, they were widely used in Pembrokeshire for stone circles and had thereby acquired some ritual association or significance. It is certain, however, that after they arrived on Salisbury Plain they were severely trimmed and their original columnar faces are no longer evident.

My guess, and it is no more at present, is that the Cardigan group of stones formed part of a stone circle in the area. Having regard to the fact that the majority of the known examples are found in pairs, it is possible that where, as in at least six cases, single stones now occur, six others may have disappeared. There may, therefore, have been about 28 stones which would be sufficient to form a circle of considerable size, perhaps about 100 feet in diameter.

A few days after the discovery of the stones near Cardigan was reported in the *Cardigan and Tivy-side Advertiser* I had occasion to pass through Eglwyswrw, Pembrokeshire, which lies on one of the main roads from Cardigan to Haverfordwest and found a considerable number of Carn Meini stones in and near the village. Some of these have been erected as gate posts in the Sergeant's Inn yard, while others alongside some of the erected stones are prostrate. Others were found strung out as gate posts on each side of the Cardigan road for nearly a mile north of the village. Some posts with a columnar structure somewhat different from that characteristic of the Carn Meini stone may have been got from other parts of the Preselly range, but the majority of the stones are undoubtedly from Carn Meini. The outcrop of that rock is only about 4 miles from the village as the crow flies and is plainly visible on a clear day. In spite of the short distance the swampy ground which intervenes between Carn Meini and Eglwyswrw, particularly along the northern foot of the mountain range, would appear to offer formidable difficulties to the transport of these great stones and it is difficult to conceive of this being undertaken in relatively modern times.

Eglwyswrw is a village with very ancient traditions and it is not unlikely that there may formerly have been one or more stone circles in the immediate neighbourhood. It is obviously a matter for further investigation which I hope to continue as soon as opportunity offers.

O. T. Jones.

## ANTLER-COMBS

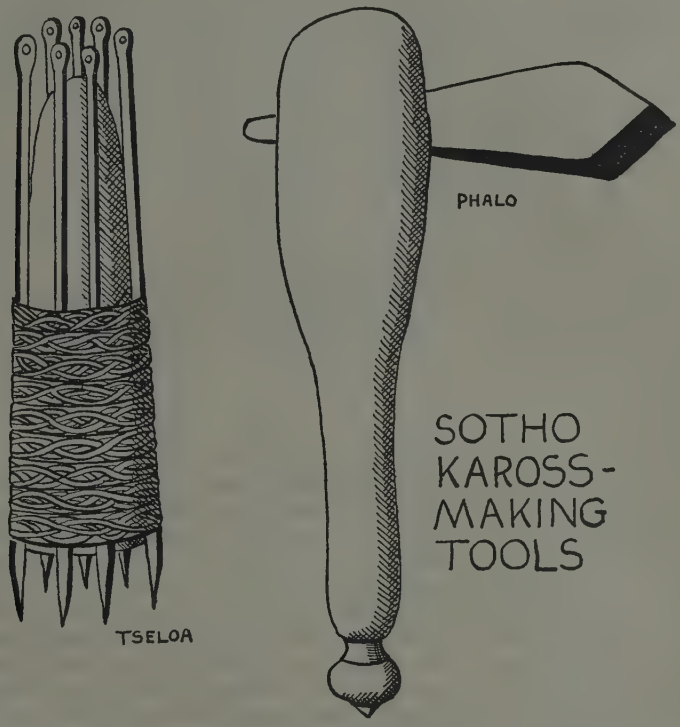
In his note on antler-combs (Antiquity, No. 115, pp. 158–9), Mr Waterer suggests that such combs could not have been used in the preparation of skins, as they would readily damage the pelt. Although it is a far cry, both in time and distance, from the present-day Sotho of Basutoland to the people of Windmill Hill, the Sotho method of preparing skins may throw some light on the problem.

Until quite recently, when blankets became universal, the Sotho used soft skin karosses as a covering for the body and the preparation of these skins was a communal task. The skin was pegged out on the ground, *ho thakhisa*, and the flesh was removed, *ho fala*, by means of an iron blade set in a handle, *phalo*.

In the next stage the flesh side of the skin was scratched with an implement termed a *tseloa* in order to make it soft to the wearer. This implement consisted of a number of long iron needles arranged around a wooden core and bound in position by plaited horse hair strands, such that the needle-points projected about half an inch beyond the base of

the core. The *tseloa* is strikingly similar to the antler-combs from Windmill Hill and Point Barrow.

This form of *tseloa*, having iron points, is most probably a development of an earlier tool, for karosses were made by the Sotho before they had much iron. Karosses are frequently depicted on the rock paintings in Basutoland and microliths are found in the same rock shelter deposits. It is possible that some of the minute scrapers and points




of the South African Smithfield and Wilton cultures were mounted as a composite tool for this purpose.

The evidence provided by the Sotho *tseloa* does not prove that the Windmill Hill antler-combs were not heckles for straightening fibres. It does prove, however, that such combs could most certainly have been used in the preparation of skin clothing and that Professor Stuart Piggott's suggestion is by no means impracticable. JAMES WALTON.

## A NEW SWANSCOMBE SKULL BONE

While excavating under licence from the Nature Conservancy, Mr and Mrs B. O. Wymer, their son John Wymer and Mr Adrian Gibson discovered the right parietal of the Swanscombe skull on July 30th, 1955. It lay in the same seam of river gravel as the two fragments previously found by Mr A. T. Marston, fifty-one feet from the occipital found in June 1935 and forty-nine feet from the left parietal found in March 1936.

The bone was in very soft condition and extensively cracked. It was removed from the gravel with great care by the excavators, assisted by Mr Marston who had been called to the scene of discovery. It was transported to the Palaeontological Laboratory of the British Museum (Natural History) where it was repaired and hardened, and photographed. The bone proved to articulate perfectly with the left parietal and occipital bones previously found.

During August, September and October, the continuation of the skull layer was systematically excavated by Mr Wymer and his associates, including helpers from the Anatomy Department of Oxford University and aided by grants from the British Museum (Natural History) and the Wenner-Gren Foundation. Numerous flint implements of the Acheulian culture, some pieces of carbonized vegetable matter, probably charcoal, and fragmentary animal bones and teeth have come to light; but so far no further recognizable human remains. It is planned to continue excavation in the Spring.

The Swanscombe Skull is the oldest human braincase in Europe, probably about 250,000 years old. That the bones are contemporary with the associated remains of interglacial fauna (e.g. *Elephas antiquus*, *Dama clactoniana*) has been confirmed by fluorine, nitrogen and radio-activity tests.

In so far as it is preserved, the Swanscombe Skull cannot be distinguished with any certainty from some primitive forms of modern man. It is not of the specialized Neanderthal type (which is much later); but the frontal bone which still awaits discovery may well have been prominent as in the Steinheim skull found in slightly later deposits near Stuttgart.

## FALSE VAULTS

Some years ago I contributed to ANTIQUITY (Vol. XXV, p. 94), a note on the false-vaulted buildings, locally called *bouries*, originally constructed by the inmates of a late medieval leper-colony established by the Cistercian monks of the Abbey of Sénanque, near Gordes, in Provence. I did not that time, however, appreciate the fact that, at least in the stonier parts of Europe, such survivals of a prehistoric style of architecture were to be found in many districts. In addition to the *bouries* of Sénanque and the well-known *trulli* of the Bari region in south-east Italy, round, false-domed huts have been constructed almost within living memory in at least two districts of south-western France, and false-vaulted huts of rectangular ground-plan in several of the Greek islands. Round Collioure, on the Mediterranean coast a few miles north of the French-Spanish frontier, dozens of such round huts, of miniature size—about five feet in height and six or eight in diameter, externally—but very solidly built, stand scattered among the vineyards and serve as shelters or, fitted with solid modern doors, as tool-sheds. In this miniature size they must be immensely durable; I was unable to find out anything about their age, except that they are no longer built nowadays. Any full-sized habitations in this style must almost everywhere have been torn down to be replaced by more modern dwellings; but near Bouziès-Haut, in the Lot department, Professor P. Grimal of Bordeaux (now of the Sorbonne), whose family comes from that district, showed me the ruins of a similar but much larger hut, some twelve feet in *internal* diameter and originally perhaps of about the same in height, with its walls still standing to a height of over eight feet and converging to form the now fallen dome, and informed me that according to members of his family it had been still inhabited in the middle of the 19th century.

In the Cyclades, particularly at Tênos, numerous rectangular huts of false-vaulted construction, with the thick, dry-stone walls curving gradually inwards until the gap between them can be bridged with slabs, are still in use intermittently as pens for animals,

though many others stand partially ruined. A good group may be seen just before one reaches the excavated remains of the classical sanctuary of Poseidon at Kionia, some forty minutes' walk along the coast road, north-west of Tênos town. The usual dimensions are about six feet by ten or twelve in internal floor-space, by six feet or less in height, giving standing room for a man of average Greek stature; walls and roofs, above the 'ceiling' slabs, are one-and-a-half feet or more in thickness. A common feature internally is a 'built-in cupboard' near the doorway, presumably a place for the shepherd to keep his goat's-hair bag containing his food and belongings. In Keos (modern Kia or Djiâ), I have seen similar sheds; and in the south-easterly part of this island, near a quarry which produces six-foot or even longer beam-like slabs of schist (?) convenient for roofing, I have even been entertained overnight in a farmhouse consisting chiefly of two fair-sized rooms, with stone benches running round the walls and stone bed-slabs in corners, in which the inward tilt of the side walls, though much reduced, and at a uniform angle, not curving, was still quite evident. Between them was a third room or loggia, open to the sun and air on the south. The great slabs which span the roof are known as *dhóki* (spelt *dokoi*, 'beams', a word as old as Homer). They rest directly upon shorter slabs from the same quarry, which project a foot and a half from the wall; these are known as *dhondia* (*dontia*, 'teeth'). The 'Mycenaean' appearance of the place was as well marked as the Homerically beautiful manners of its inhabitants. (Incidentally, it seemed only natural that a son of the family, who had just, as the Greeks say, 'come out an officer' in the army, gave me on the boat from Piraeus an account of the secret history of the Yalta conference and the beginning of the Cold War, interpreted entirely in terms of the personal motives of leading statesmen, which as a story would have done no discredit to Herodotus; a charming product of a keen and completely unsophisticated Greek mind based no doubt on the talk of his cadet college. I would gladly have taken notes; but in a small ship in a rough sea I was holding on with both hands.)

It was more unexpected to find that some of the miniature island churches, not much larger than the 'Mycenaean' cow-houses at Tênos, are built in the same manner; a fact usually concealed by plaster and whitewash, and only to be observed on the rare occasions when a church falls into disrepair. This has, however, happened to at least two of the 365 (or 370) churches of Mykonos, which abut upon ancient remains at the site known as Portés, about 40 minutes south of the little town. To the distress of the local peasants, repairs have been at least temporarily forbidden, for fear of further damage to the antiquities, some stones of which may already be seen built into the church walls.

The above scattered observations, made during holiday journeys primarily concerned with Greek and Roman antiquities, could probably be multiplied. They are enough, however, to suggest that the survival of this prehistoric method of building down to post-medieval times was widespread in southern Europe, and to suggest the questions, How late are the latest of the 'hut-circles' so plentifully marked on our ordnance maps in Britain? How common were false-vaulted cottages, in regions where stone was easier to come by than good beams for roofing, throughout classical and medieval times? It seems as though such buildings, unmentioned in literature and probably despised as merely the primitive work of a despised and backward peasantry, may have been common throughout wide areas. It is a commonplace today, that 'primitive' agricultural methods, agrarian rituals and pre-rational beliefs have survived into modern times in Europe as well as in other parts of the world; and in this context it is not without interest to speculate as to how many peasants' dwellings, as well as their habits of thought, may long have remained almost indistinguishable from those of their ancestors in the Bronze Age.

A. R. Burn.

## A NEW INSCRIBED CLAY TABLET FROM ENKOMI

In the summer of 1955, during supplementary investigations in the Late Bronze Age town of Enkomi by the writer for the Cyprus Department of Antiquities[1] in the area of the north sector of the City Wall, a new fragmentary tablet of baked clay with a Cypro-Minoan inscription was discovered (PLATE IX). It was found in a Late Cypriot I 'fort' adjoining on the inside the north sector of the City Wall, which wall dates from the latter part of the 13th century B.C.[2] The fort was constructed very early in Late Cypriot I, if not slightly earlier, that is to say in the early 16th century B.C. It was a large rectangular structure with strong external walls enclosing many rooms. At an early stage of its history the fort was altered and remodelled and later at the end of the Late Cypriot I (end of the 15th century) it was abandoned and even partly destroyed by people who quarried in its remains for stone and other materials. In the early part of the Late Cypriot II period, i.e. early 14th century B.C. the abandoned fort was overlaid by another elaborate building complex which lasted to the end of the 13th century, when it was finally abandoned. The City Wall had been built along the north wall of the Late Cypriot II complex a short time before its final destruction and abandonment.

The tablet now published was found in a room of the Late Cypriot I fort under floor VIII, counting from the top, which belongs to the period of remodelling. Actually the earliest floor of the fort was floor XI, but at the time of floor X, i.e. soon after the original erection, certain changes were carried out and to this altered fort floors IX–VI can be attributed. The tablet was found in a filling between floors VIII and IX and was therefore not in its original context. The pottery found in the same filling consists chiefly of white slip I, base ring I, monochrome and other wares which are typical of the Late Cypriot I period. A feature worth mentioning is that following the alterations on floor X we noticed the installation, in the room in which the tablet was found, of copper workshops evidenced by a large heap of carbonized matter, fragments of crucibles and tuyères used in furnaces.

The tablet therefore belongs to the second period of the fort, but fairly early in its history. If we remember that it was found in a filling between floors VIII and IX only an *ante quem* date can be given, i.e. the date of floor IX at the latest, in other words some time at the end of the 16th or the beginning of the 15th century B.C. This is very important as it shows the use of the Cypro-Minoan script at least a century earlier than the appearance of Mycenaean traders at the end of the 15th century.

The early appearance of the script in Cyprus was already noticed by the late J. F. Daniel of the Curium Expedition[3] who attributed its first appearance at Curium to the Late Cypriot IA : 2 period, i.e. about 1500, exactly as at Enkomi. Daniel had only discovered short inscriptions mostly on clay vessels but here, at Enkomi, we have now

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[1] The two tablets previously found (1952–3) in the same excavations were published by the writer in a preliminary manner in ANTIQUITY, vol. XXVII, pp. 103 ff. and 233–7. The second of the two has been deciphered into Greek by E. Sittig: *Nouvelle Clio*, 1954, pp. 470 ff. The excavations at Enkomi form part of a joint expedition, the other component being the French Mission led by Dr Cl. F. A. Schaeffer, to whose publication, *Enkomi–Alasia*, vol. I, Paris, 1952, the reader is referred. Dr Schaeffer found a similar tablet in his 1953 Enkomi campaign and has reported the discovery of a Cypro-Minoan tablet in his excavations at Ras Shamra–Ugarit. (ANTIQUITY, vol. XXVIII, pp. 38 ff). The total of tablets found at Enkomi including the one found by Dr Schaeffer is therefore four.

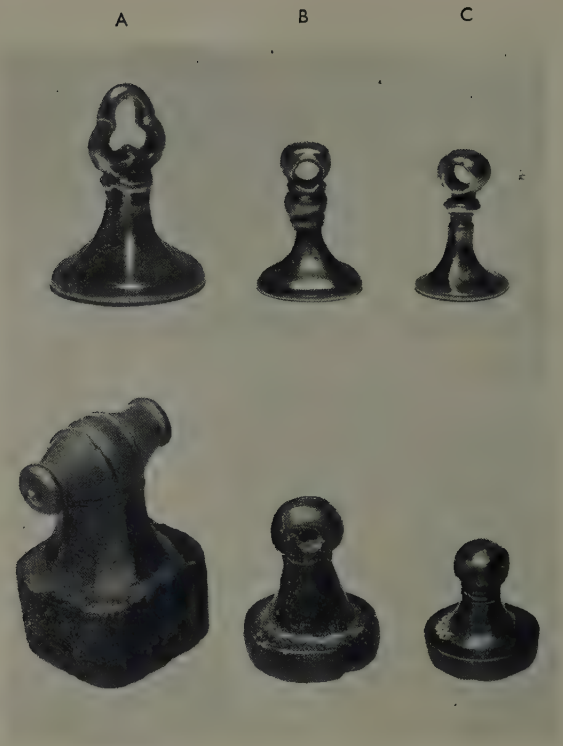
[2] *J.H.S.*, vol. 75, 1955 Supplement, p. 29, fig. 1.

[3] *Vide* 'Prolegomena to the Cypro-Minoan Script', in *A.J.A.*, vol. 45, 1941, pp. 249 ff.

PLATE IX

INSCRIBED CYPRO-MINOAN CLAY TABLET FROM ENKOMI, CYPRUS
(see p. 40)

# PLATE X




*a, b, c.* MEDIEVAL ENGLISH SEALS $(\frac{1}{1})$
*d, e, f.* HITTITE SEALS (slightly enlarged)

discovered the first long text testifying indubitably to the extensive use of the Cypro-Minoan script in Late Cypriot I and confirming the correctness of Daniel's observations.

The fragment here described is the upper part of a rectangular tablet the total height of which is unknown. Its width is 7.5, its preserved height 6 and its thickness 3.3 centimetres. It is of hard baked buff-pinkish clay and is inscribed only on one face the preserved inscription being the top three lines. Moreover on the narrow sides appear individual signs one of them, the left one, being damaged. Above the sign on the left narrow side a narrow horizontal hole may have been produced by a metal pin, with which the tablet was held during the engraving of the inscription. The inscribed face and the sides are well smoothed but the back or reverse is somewhat uneven and partly worn. The shape of this new tablet is therefore different from the pillow-form of these previously discovered at Enkomi and indeed nearer the form of the Cretan–Helladic types, although thicker. Another feature which calls for comparison with the Cretan or Helladic tablets is the presence of interlinear dividers which do not appear on the tablets previously found at Enkomi and which belong to the later part of the 13th century. (See also Dr Ventris' notes below).

The signs were engraved with a pointed tool which usually produced evenly deep incisions; sometimes, however, these become deeper at the middle of the strokes or thin off towards their upper or lower ends. This represents a style different from that of the previous tablets where the strokes are shorter and almost 'cuneiform'. The size of the signs is 7 to 10 mm. as against an average of 3 mm. on the previous tablets.

As already stated the script is Cypro-Minoan[4] and I am very grateful to Dr M. Ventris for communicating the following notes which he has kindly prepared.

P. Dikaios.

## Notes on the Enkomi 1955 Tablet

The script of the 1955 tablet, while presumably ancestral to that of the 1952–53 tablets from Enkomi, presents many more specific similarities with the Cretan linear scripts, and in particular with Linear A. Chadwick is perhaps more confident than I am in deducing a direct connection (I feel it is premature to construct family trees of early scripts while the evidence for their distribution is so sketchy), but certainly of the 21 (?) different signs on this tablet about 10 can be directly paralleled in Linear A. The first sign in line 3 is particularly close to the Agia Triada sign used both phonetically (=Mycenaean *ni*) and as an ideogram for FIGS : in the absence of numbers it is presumably phonetic here. It is noteworthy, incidentally, that few if any of the signs on this tablet repeat themselves.

I do not see any clear evidence for word-dividers (which are obligatory both in the linear scripts and in the later Cypriot inscriptions); nor for the determination of the direction of writing, though I feel that left-right is more probable. If so, the third sign of line 1 faces the same way as the Cypriot syllabary *ta*, but the opposite way to Mycenaean *da*; while the penultimate sign of line 2 points the same way as Mycenaean *se*, but the opposite way to Cypriot *se*. This might be a (doubtful) argument for reading the tablet *boustrophedon*. Both these signs recur in their Mycenaean directions on Enkomi 1953 (Nos. 2 and 11); the oblique angle to the side stroke of *ta* (?) on the 1955 tablet can be paralleled on Linear A tablets but not on the Mycenaean.

[4] Apart from Daniel's *Prolegomena* see also O. Masson 'Nouvelles inscriptions en caractères chypro-minoens' in Cl. Schaeffer's *Enkomi–Alasia I* and also 'Epigraphie chypriote' in *Orientalia*, vol. 23, pp. 442 ff.

From their irregular course the interlineal dividers (absent on the 1952–53 tablets) seem to have been drawn after the text rather than before. We are not clear on the function of the raising of the three signs in the centre of line 1 (a word in parathesis, as often in Mycenaean ?), or on the sequence of writing that led to the doubling of the interlineal underneath them. The script of the terracotta balls from Enkomi is presumably intermediate in development between the 1955 and 1953 tablets. Note the wheel-shaped sign at the beginning of line 2 (identical with Mycenaean *ka*), and compare it with the ' square with a cross in it ' of an Enkomi ball (*Palace of Minos*, IV, 760) : this perhaps develops into the simple squares of Enkomi 1953 (Nos. 32 or 36). M. VENTRIS.

## SEALS

The use of personal seals can be traced a very long way back in Eastern countries, where it still continues. In medieval Britain the custom was widespread and necessary for documentary use.

The developed 'Gothic' type (PLATE X, A) was long universal, but by the 17th century most examples had become reduced in size, although the general hexagonal or octagonal form persisted ; the pierced trefoil handle had however assumed a more simple and rounded form (PLATE X, B, C) and this type, the ' chess pawn ', with slight variations, is occasionally found up to the 19th century. The other changes of form are outside the purpose of this note, but the chess pawn held the field in the middle years of the 17th century and it is safe to assume that it was with small seals of this type, usually made in silver, that King Charles's death warrant was signed.

It was, therefore, with considerable surprise, as a collector of English seals, that I recently noticed a pair (PLATE X, E, F) in the Ashmolean Museum. It will be seen that they reproduce the chess pawn type very closely, the main difference being the thickness of the base. The material is haematite, but the type is sometimes made in metal.

PLATE X, D, E, F are from D. G. Hogarth's *Hittite Seals*; Oxford, 1920 (p. 21, fig. 18, see also p. 38, no. 1197 and pl. VII) and it is there stated that ' the handle is . . . usually horizontally grooved where it springs from the base and again where it expands into a spherical knob, which is . . . always pierced with a suspension hole. The stem of the handle is also often facetted. The base of all specimens known to me is circular (sometimes with scalloped edge) and flat faced . . .' The smaller specimen (PLATE X, F) is said to be from Cilicia.

As to date, Mr W. L. Brown tells me that ' a safe date would be second half of the second millennium B.C.', i.e. some 3000 years before the type reappears in England.

What process can have brought about this Hittite form, so strangely similar to the English and with no known—or at any rate no recorded—pedigree, although the medieval type has a perfectly clear pedigree of its own ?

The resemblance to other contemporary Hittite seals is comparatively faint and Dr Hogarth does not suggest any process of development. PLATE X, D has the same fluted shank and collar, and the barrel-shaped knob may precede or follow the simple ring of the smaller pair.

Although Dr Hogarth admits the possibility of forgery in the case of some Hittite seals, it is scarcely likely that a forger would adopt a form without precedent.

PLATE X, A is of brass and 14th–15th century date with a shield of arms and the inscription ✱ · S' · IOHANNIS · DE · DEYN.

PLATE X, B is of silver, 17th century, arms not identified.

PLATE X, C is of silver, 17th century type, with arms of a lady of the Sykes family.

W. J. HEMP.

# Reviews

ROMAN ROADS IN BRITAIN, Vol. I. *By* I. D. MARGARY. *pp.* 255, 17 *plates and* 11 *maps. Phoenix House.* 42*s.*

Mr Margary's earlier book, *Roman Ways in the Weald*, set a new standard in the tracing and recording of Roman roads in this country. In his preface to it, he expressed the hope that it might lead to improved regional surveys elsewhere, and although none has yet appeared its influence has been seen in several accounts of single stretches of road published in county journals. In the same preface he remarked that ' it is unlikely that anyone could now rewrite Codrington successfully on a detailed scale '. That Mr Margary has himself undertaken what is virtually a new edition of Codrington need not be contradictory. Although slightly more detailed than Codrington's, this new survey is still deliberately superficial and in no way reduces the need for the series of local studies. Indeed any assessment of its value must depend on how far it is likely to stimulate and assist such studies in the future.

The book is divided into six chapters, of which the first is concerned with generalities and the remainder deal, area by area, with the roads lying south of the Fosse Way and the Bristol Channel. The arrangement is simple and clear and the text is supported by regional sketch maps, a general map, and an excellent reference system which must surely become standard. As the author anticipates, not all his omissions and inclusions will command universal assent, and among the latter one may regret the unqualified acceptance of Ashwell Street and the absence of any reference to Crawford's work on it. In general one could wish that he had been freer with the dotted line on his maps—the southern end of the Fosse Way, for example, still needs elucidation—and also allowed himself more scope in the inclusion of (properly indicated) doubtful routes. Throughout one feels that space might have been saved by regular map references, but equally one is lost in admiration at the way in which Mr Margary avoids monotony and contrives to describe a road's course in really readable English. Most of the plates are magnificent.

But if this is to be regarded as the potential master volume of a series, the most important part of it must be the introduction. This is too short—little more than half the length of the introduction to *Roman Ways*, though there is nothing there that is not relevant here, and much that needs to be added. Secondly, it shares with the earlier work a certain unconcern regarding the relationship of Roman roads to other Roman remains, a relationship which is of primary importance in the effort to discover their course and which gives its ultimate purpose to the exercise. A corollary of this is a lack of interest in date. If ' the lay-out of the roads was generally undertaken at first in response to military requirements for the conduct of a campaign of conquest, or for the effective policing of a country after its subjection ', why are there no roads to Hod Hill ? The generalization may be true, but the evidence for it in southern Britain will only be built up slowly by the observation of sections, and these are most likely to be helpful when they are cut near occupied sites. Again, of the seventeen sites within the area of this volume where milestones have been found, only one is mentioned, along with the very dubious ' bull-baiting stone ' at Stinsford. These too can have historical value. Is it sheer accident that while we have three stones of Hadrian we have none at all of an earlier date ? This was clearly one of the things which Hadrian ' corrected ' in Britain, but what was the state of the Fosse at Thurmaston before he did, or caused the Coritani

to do, whatever was there commemorated ? Milestones are surely proper objects for study by road-workers—who else will look in the right places for inscriptions on gateposts and rubbing-stones ?—and the omission of the Cornish group involves the evasion of one of the major road problems of Britain.

Mr Margary is most at home, and so most helpful, on the laying out and construction of roads, and here he allows himself some speculation. A system of movable beacons may be adequate to account for some alignments, but hardly for the most impressive of all, that of Stane Street on Chichester and that of the Gartree Road on Colchester. These seem to imply a more thorough survey, possibly that which gave Marinus his remarkably exact figure of 59 Roman miles from London to Chichester. Mr Margary explains the use of an unnecessarily large *agger* as perhaps designed to impress the natives. No one who has observed the flamboyant behaviour of Mussolini's roads in Abyssinia will be disposed to reject this out of hand, but here again the historical context needs to be considered : Ackling Dyke was presumably built after Hod was evacuated.

Following Codrington, Mr Margary refers to the itineraries, but his treatment of them is uneven. Extracts from the Antonine Itinerary are given at the end of each section, with the true distances in English miles ; this seems unnecessarily confusing, but the Roman mile is nowhere mentioned. The nature of the Itinerary itself is barely touched on ; and while it might be rash to connect it dogmatically with the Cursus Publicus, that institution deserves mention in its own right in a treatise on Roman roads. The Peutinger Table is used in East Anglia, but the implications of its truncated names are not pursued south of the Thames. *Ridumo* is (*Mo*)*ridunum*, still 15 miles from Exeter, and so not Seaton ; *Madus* is surely (*Novio*)*magus*, 17 miles from Rochester, and since this, added to the Antonine figure of 10 from London, gives the Antonine total of 27, the name can be firmly anchored near Crayford ; and the amount of redrawing implied in all this suggests that the looped road shown between Dover and Lympne is not ' exceptionally strong authority for assuming its existence ' in fact, but simply the ill-assimilated remains of the two roads from Dover and Lympne respectively to Canterbury. Finally the Ravenna Cosmography, as treated by Richmond and Crawford, does more than confirm what we know from other sources. Among other things it seems to suggest a west-east route across Dorset passing to the north of Dorchester. In no case is the reader told where the best texts are to be found.

Mr Margary is hardly more forthcoming on the subject of later documentary evidence. He mentions the importance of Saxon charters and the value of place names in general, but not the fact that time can be saved and errors avoided by the use of the growing body of published research into their origins. Reference to *E.P.N.S. Essex*, for example, reveals that the street name Watling in Thaxted is connected not with a Roman road (which should pass well to the east of it) but with the possessions of John Walkelyn. Indeed the bibliography of modern works as a whole is very thin. At home one most regrets the omission of the relevant chapters of *Archaeology in the Field*, but equally serious is the total absence of any references to comparative material from the rest of the Empire.

Many of these omissions appear to be deliberate and are perhaps due to an underestimate of the stomach which that chimerical character, the general reader, has for such things. But, as the publisher's blurb assures us, the work ' has been undertaken with the support of the Council for British Archaeology, as a research project of importance ', and (particularly in view of the price) it is necessary that the student should know what he is and is not getting. What we have here is a very readable, well-arranged and well-produced account of the courses of a number of known Roman roads ; and in fact this

is all Mr Margary has set out to give. What we have not is a comprehensive work on Roman roads in general and in all their aspects. This is still required by the archaeologist and the historian alike. A. L. F. RIVET.

RELATIVE CHRONOLOGIES IN OLD WORLD ARCHAEOLOGY. *Edited by* ROBERT W. EHRICH. *pp.* 154. *University of Chicago Press*, 1954. (*Cambridge U.P.* 19*s.*)

The nine papers here published were read at a symposium at Philadelphia in 1952 ; only those of Kantor and Goldman seem to have been revised down to 1953. Though available radio carbon dates are mentioned, absolute dates have generally been avoided as the several contributors are not entirely agreed on the historical chronologies of early Egypt and Mesopotamia. Kantor, Braidwood, Perkins, McCown, Goldman, Weinberg and Ehrich set forth their respective views on the culture-sequences in Egypt, Syria, Mesopotamia, Iran, south-eastern Anatolia (Cilicia), the Aegean and south-east and central Europe, expound tentative correlations with adjoining provinces and recapitulate their results in tables generally illustrated by rough sketches. Albright's paper on Palestine is little more than a commendatory comment on Kantor's while Ward outlines cautiously a sequence for China without venturing on tabulation. All papers are accompanied by comprehensive bibliographies. The editor had provided a wise introduction and an invaluable index.

The sequences within each province are inevitably based very largely on ceramic evidence and so are many of the correlations with other provinces, particularly in the papers by Goldman and McCown. In the discussion ' some concern was voiced over the heavy emphasis on pottery '. Of course within a unitary cultural province the stratigraphical succession of ceramic styles provides a delicate and unimpeachable basis for periodization. But if such a province embraces at all an extensive tract, the chronological framework thus provided is likely to become distinctly loose. Even along the Tigris, for instance, there is good evidence that Ubaid pottery appeared later at Gawra in the north than at Eridu in the south. Where, as in the Orontes Valley and Cilicia the cultures, represented by Halaf and Ubaid wares and presumably starting nearer the Tigris than the Middle Euphrates, intrude into regions previously occupied by quite other assemblages, the precision of the synchronism must be still further diminished. Actual imports of course do give precise synchronisms. Yet, though imported Halaf and Ubaid vases did in fact reach Cilicia, they were probably manufactured no nearer the starting point than the Orontes valley or Ras Shamra.

Wares less sharply defined are no better chronological fossils than metal types or battle-axes. Pattern-burnished wares are used by Weinberg to define a Late Neolithic horizon round the Aegean. Now in the Levant such wares are reported from the deepest pre-Halaf levels at Ras Shamra and Judeideh. Yet pattern-burnishing is characteristic of the late grey wares of northern Iran, occurs in Uruk levels at Kish and re-emerges in a modified form in the ' lattice-burnished ware ' of Palestinian E.B. II. Weinberg takes these later parallels as the basis for his Aegean synchronisms, but only because they thus fit best into a general pattern. This is re-inforced by equally good—or bad—parallels with the Ghassulian of Transjordan that is put by Albright rather earlier than the Esdraelon culture whose grey burnished bowls are so strikingly like Cretan neolithic and Uruk wares. Yet synchronisms based on locally made pins or weapons are no better. For Weinberg double spiral-headed pins mark an horizon equating Troy IIg, the ' Royal Graves ' of Alaca, the ' Royal Tombs ' at Ur and Hissar II-B to III-A. McCown on the strength of pottery equates the transition from Hissar II-B to III-A with that from

Protoliterate (Late Uruk) to Early Dynastic in Mesopotamia. Most contributors, including Weinberg and apparently McCown, assign the Royal Tombs of Ur, or at least the metal types characteristic of them, to the Akkadian period and so to the time of the VIth Dynasty in Egypt because 'the Akkadian period is little known culturally in southern Mesopotamia while the metal types generally associated there with the Royal Cemetery occur only in an Akkadian context in the north'. Of course the 'Royal Tombs' at Ur as at Alaca are only 'contemporary' in the same sense as those of our Hannoverian sovereigns. And, if Woolley's division of the Ur graves be not accepted, many of the relevant metal types occur already in the Y cemetery at Kish that must be Early Dynastic I and perhaps five centuries before Sargon of Akkad! The fact is, these metal types enjoyed an annoyingly long life, presumably longer in provincial areas than in metropolitan Sumer, and so can support quite divergent chronological correlations.

Apart from these discrepancies, those authors whose conclusions can be compared at all have independently arrived at correlations that, at least from the Tigris to the Nile and the Adriatic, agree satisfactorily with one another and for that matter with my own of 1952. It is very convenient to have the conclusions of these American specialists presented in a single volume together with a summary of the data on which they have relied. They make also many original points of which only five can be noticed.

'The seeming similarity' between the crude Egyptian bowls of Petrie's classes R and L and the Mesopotamian bevelled-rim bowls characteristic of the Protoliterate period 'is purely illusory', the Mesopotamian type has not been found west of Hama (p. 6). 'The grey ware people of Hissar II should be considered as a possible source for the Uruk ware' (p. 47) and for its Cilician analogues (p. 70) rather than Anatolia. Imported sherds of Cypriote Erimi and Alonia wares (transition from 'Neolithic' to Early Bronze) occur at Tarsus in the same context as vases exported to Egypt and datable there about 2650. Impressed spirals from Ghassul may explain Cycladic spirals without appeal to Danubian influence (but after all there is a much older spiral stamp from Jarmo). Ehrich, who assisted in the excavation, can find no stratigraphic indication that painted ware at Starčevo appeared later than the rough unpainted fabrics (but Mrs Garasanin, who was also present, has just re-asserted the latter's priority).

V. Gordon Childe.

CURRAN POINT, LARNE, COUNTY ANTRIM: the type site of the Irish Mesolithic. *By* Hallam L. Movius, Jr. *Proceedings of the Royal Irish Academy, vol. 56, section C, no. 1. 195 pages, 20 text figures, 11 plates. 25s.*

With appendices by Knud Jessen (Palaeobotany, W. S. Benninghoff (Sedimentology, Arthur Earland and Mrs Fisher McMillan (palaeontology) and Madame Lemoine (calcareous algae), H. L. Movius gives a somewhat late, but very thorough account of the 1935 excavations, by the Harvard Archaeological Expedition, at Curran Point.

Larne, some 18 miles north-north-east of Belfast is a classic site of the Irish Stone Age. The Harvard excavations were made at Curran Point, a low, long, sickle-shaped spit. The deposits consist of stratified beds, formed under intertidal conditions. 'The main purpose of the excavations were (a) to check pre-existing data; (b) to obtain a large serie of documented archaeological specimens, and (c) to secure additional information concerning the nature of the Early Post Glacial sediments'.

After the Late Glacial transgression, approximately 50 feet above present sea level was a first period of Post Glacial emergence (120 feet), then a post-Glacial submergence (25 to 30 feet) during the time of Jessen's palaeobotanical sub-zones VI b, VI c, and VII a. The most extensive deposit of this stage is the so-called 'Estuarine clay', which, from its

fauna and petrography, is neither estuarine nor a clay. In addition, current bedded and intertidal sediments (gravels and sands) were accumulated. These have been incorrectly referred to as ' raised beaches ' and contain mesolithic implements of the Larnian Culture, in secondary position. A second period of emergence followed, with formation of storm beaches, which contains rolled Larnian artifacts. Since the beginning of the last century, a slight submergence seems to occur in this area.

The excavated site (a pit 5 m. square) shows the following stratigraphy : (a) Surface accumulation (0 to 0.40 m.) flint implements, derived from (b), mixed with bricks, china, etc. (b) Storm beach material (0.40 to 1.30 m.) with numerous flint implements, heavily rolled for the most part. (c) Inclined beds of gravel and sand, deposited under conditions of sinking coastline (1.30 to 4.25 m.). Abundant implements. (d) Fine reddish brown sand (4.25 to 4.80 m.), with 25 implements, rolled or unrolled. (e) Hard grey sand (4.80 to 5 m.), with several unrolled implements. (f) Fine reddish brown sand (5 to 5.15 m.), with one flake. (g) Dark blue sand (5.15 to 6 m.), deposited in shallow water. No implements. (h) ' Estuarine clay ' (thickness unknown), with five flint artifacts.

This ' estuarine clay ', on the basis of the Mediterranean foraminifera which it contains, was deposited under warmer conditions, the temperature being ' comparable with that of the southern shore of England '. At Magee Island, in front of Curran Point, a corresponding layer has been dated of the lower part of sub-zone VII a (climatic optimum). So the maximum warmth was reached before the maximum of the post-glacial marine transgression, which took place during the transition from sub-zone VII a to VII b (Atlantic Sub-boreal) in Ireland.

A total of 5,515 artifacts was obtained from the pit. After criticizing the typological approach in the study of ' prehistoric cultures ', Movius used it himself, for, in that case, nothing else is possible, of course (there would be plenty to say about the ' sociological approach ', too). He remarks that the old finds at Curran Point have been misleading about the general facies of the Irish Mesolithic, the specimen having been picked up from the beach, and only the big ones collected, the light ones being washed away by the waves. The results of the Harvard Excavations sustains this contention. Misleading also would be a classification by ' état physique ' (rolling, patina). On the other hand, the results of the excavation cannot be regarded as giving a good stratigraphical sequence, the most part of the implements being certainly derived, in secondary position. So Movius studies them as a whole.

Percentages are given of each recognized type of tools, classified also by size and rolling. But to count the waste flakes and the cores together with the tools seems to us an error. There are big flake implements, flakes and blades, scrapers (436), points (82), perforators or awls (134), Larne picks (101), cores (142). The scrapers (in a very wide sense, and not all of them very convincing) are side scrapers (21), concave scrapers (53), notched scrapers (77), end-scrapers (66) (some seems to us more likely to be truncated flakes), concave-ended scrapers (56), steep-scrapers (12) (*not* true steep-scrapers of Upper Palaeolithic types, as the author says with reason) and, the best among them, thumbnail scrapers (151). The ' points ' are roughly pointed flakes rather than true points. Perforators seems generally good. The ' Larne pick ' is ' the type implement of the Irish Mesolithic '. They are flakes with the bulb of percussion at the narrow end, the opposite end being broad and thick. They have been used by the bulbar end. ' Prior to detaching these peculiar implements, the core has been carefully prepared, often by the removal of short flakes which have broken off before reaching the end of the core opposite

the striking platform '. ' Experimentation has shown that it is extremely difficult to manufacture these implements intentionally ', says the author. On the contrary, we believe that this kind of flake is common enough in Upper Palaeolithic and Neolithic cultures, but has been generally considered as ' waste flakes '. And, experimenting on blade technique, we have often made them, voluntarily or non-voluntarily. The only difference seems to be that, in the Larnian culture, they have been used at the bulb end.

No axe or celt was found in the Harvard Excavation. The author examines the evidence for occurrences of axes or celts, which, in the Late Larnian, points to an influence from the Mesolithic Forest Culture of Northern and North-western Europe.

The cores are relatively rare at Curran Point, two ' microliths ' and a ' graver ' seems, to say the least, dubious, as the author observes himself.

The conclusion is that this ' Late Larnian ' of Curran Point is a flake-blade complex, ' which developed with certain modifications from the Early Mesolithic of Ireland. But core tools—choppers, axes, celts, adzes and picks—chipped from flint nodules, also occur '. Owing to the conditions of deposition, no true stratigraphical sequence can be worked out at Larne.

These conditions of deposition rules out, also, the possibility that the site was ever actually occupied by the people of the Late Larnian culture. Although some ' fresh ' tools can have been manufactured on the spot, the majority of the implements must come from ancient ' midden ', not far from the site, which have been destroyed by the sinking of the land.

The Early Larnian is typologically nearer of the Creswillian of Britain than of the Late Larnian, ' a fact which reflects environmental conditions '. In the Late Larnian, coastal type of existence, based on collecting edible molluscs (Post-Glacian optimum resulting in an exuberance of molluscan life) and fishing supersedes the ancient hunting life of the Early Larnian. The implement types, crude and generalized, with few retouched forms, indicate a peripheral culture, which has lost its vitality, a ' strand-looper ' type of existence.

After a discussion of Mesolithic development in south-western Scotland, and a study of the age and affinities of the Obanian culture—which is definitely *not* Azilian—Movius studies the cultural development in northern Britain and Ireland during the Sud-Boreal times, the Larnian survivals in the Lower Bann valley, and the cultural affinities of the Larnian. It shows no definite influence by the Tardenoisian, the ' micro-burins ' found at some places existing already in the ' developed Creswillian ' of Lowland Britain. In the Obanian (which Movius takes as a regional development of the Early Larnian), frequent references have been made to the existence of Forest Culture influences. But these cultural traits (axes, antler tools, etc.) were probably spread by diffusion rather than by true Maglemosian settlements in the Larnian-Obanian area.

So, ' the various Mesolithic assemblages of Ireland and Scotland, known as Larnian and Obanian, are indigenous entities with their own growth and life, and they cannot be explained on the basis of a blending of different cultural elements introduced by successive waves of migrants from the south '.

The appendices and a good bibliography ends the book.

As a whole, to a non-specialist of British Mesolithic, this work seems to be a very complete and thorough monograph, which deserved a better material than the rather poor assemblage found at Larne.

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HOËDIC: DEUXIÈME STATION—NÉCROPOLE DU MÉSOLITHIQUE COTIER ARMORICAIN. *By* MARTHE *and* SAINT-JUST PÉQUART. *pp.* XXII, 93; X *plates*, 42 *figs. Antwerp, De Sikkel*, 1954. *Price* (*paper cover*), 280 *Belgian francs.* (*Obtainable from Garraway Ltd.*, 11 *Kensington Church St., London, W.*8. £2 1*s. including postage*).

The present well-produced volume represents the long-delayed final report of the important mesolithic station on the small island of Hoëdic, south-east of the Quiberon Peninsula in Morbihan, excavated by the surviving author and her husband between 1931–4. The site resembles in general character that explored by the same authors on the island of Téviec off the west coast of Quiberon between 1928–30 and sumptuously published in 1937 as *Mémoire* 18 of the *Archives de l'Institut de Paléontologies Humaine.* Both sites comprise midden deposits incorporating numbers of outstandingly interesting burials and at the time of their occupation by Tardenoisian man it is likely that both formed part of the mainland (to which they would be joined when sea-level was less than 20 metres lower than it is today). It is not surprising therefore that in all essential respects the finds from Hoëdic repeat those from Téviec.

The excavation at Hoëdic, however, provided particularly important confirmation of the work down at Téviec, since the midden deposits at the former were masked by an undisturbed deposit of sand up to two metres thick. The whole of the site was cleared and it is noteworthy that its total extent was approximately 200 square metres, round about the same size as Star Carr. The numerous half-tone illustrations of the site have, no doubt for reasons of economy, been reproduced at an inadequate scale. Owing to the similarity of the finds to those from Téviec, these have been summarily dealt with, though a number of excellent illustrations are given. On the other hand the nine burials, containing remains of thirteen individuals (nine adults and four infants), are described in detail as well as being illustrated. Summary notes on the skeletal remains are given by Professor Vallois, who was responsible for the full description of the twenty-two individuals from Téviec.

Taken together with the sites of Téviec and Hoëdic afford the clearest picture we have of life on the Atlantic sea-board of western Europe towards the close of mesolithic times. They provide an insight not merely into technology, but also into subsistence, settlement, personal adornment, burial-customs and physical type. The publication of the Hoëdic station makes a contribution to European prehistory subsidiary to that of Téviec, but is important both as confirming the findings made at this site and also for a number of details particular to itself. J. G. D. CLARK.

EARLY ROME. I: Stratigraphical Researches in the Forum Romanum and along the Via Sacra. *By* EINAR GJERSTAD. *pp.* 164 *with* 147 *figs. Lund: C. W. K. Gleerup.* 1953. *Sw. Cr.* 60.

This is the first part (out of six) of a survey designed to 'present the entire archaeological and written evidence bearing upon the early history of Rome from the beginning of the pre-urban epoch to the end of the Regal period'. The author's approach to his problems is well indicated by an observation in the preface. 'The archaeological material' he writes 'may serve as a touch-stone in determining the intrinsic value of the record as a historic source of knowledge . . . In the hypothesis, first put forward by Pinza, regarding Rome to have been founded by an act of *synoikismos* of pre-existing independent villages, there is an intermediate stage between the isolated village-habitation and the unified city, the Septimontium stage. I must confess that I have been sceptical about the historic reality of this stage, . . . hitherto based exclusively on conclusions

drawn from the Septimontium festival, . . . Archaeological discoveries have, however, convinced me that the Septimontium stage is not a fiction but a concrete fact: . . .' I have quoted this passage not only for its interest as showing the author's outlook, but because the discoveries in question, the huts in the valley, that became the Forum Romanum, form the subject of this volume. Two sites, both originally explored, but not published, by Boni, are considered. Beside the Equus Domitiani, near the centre of the Forum, the author undertook supplementary excavations as a check on Boni's work. The second site lay further south beside the Sacra Via, and formed the stratum above the well-known archaic cemetery.

Gjerstad pays a well-deserved tribute to Boni, whose unpublished observations and records are sufficiently full and accurate to show the results obtained. His own researches, carried out with meticulous accuracy, demonstrate the excellence of the earlier work. In particular it is instructive to compare the two sections of the trenches beside the Equus Domitiani and to note how slight are the divergences.

On both sides the settlement sequence shows huts of the 2nd and 3rd pre-urban periods. (The huts on the Palatine go back to the first pre-urban period: ANTIQUITY XXV, 28). The huts are followed near the Equus Domitiani, by three levels of pebble pavements and beside the Sacra Via, by the 'habitation of developed architecture'. This second stage is firmly tied by imported pottery to the period *c.* 575–*c.* 450 B.C. and corresponds to the stage previously established in the Comitium (*Opuscula archaeologica*, vol. II). All three levels of the second stage belong to the city of the Regal period. The huts beside the Sacra Via overlay the cemetery. Late graves, coeval with the huts, are shown to be those of infants buried within or alongside the dwellings at a period when the cemetery, which included graves of adults, was already disused. The absolute chronology of the pre-urban strata is reserved for consideration in a later volume; in the meantime readers are referred to the author's preliminary remarks in *Bullettino Communale*, LXXIII, 16. C. A. RALEGH RADFORD.

ATTIC VASE PAINTINGS IN THE MUSEUM OF FINE ARTS, BOSTON. Part II *by* J. D. BEAZLEY, *with* 104 *pages* and 12 *plates, and a separate portfolio of* 34 *plates. Oxford University Press*, 1954. £8 8*s.*

The original plan of this work was to include 'all the Attic vase paintings in the Museum of Fine Arts which deserve to be reproduced at their actual size' and this volume carries the task two-thirds of the way to completion. The policy of the publication of drawings and some photographs at life size entails, as it did for the first volume which appeared in 1931, a large portfolio of plates which measures 23½ inches by 17½ inches, not an easy format to use or one a student would welcome in many publications; but this does not set a standard to be observed in all publications of Attic red-figured vases, only in those of the best vases of the richest collections, and in this case the excellence of the drawings and photographs and standard of reproduction far outweighs considerations of handiness. The text includes some drawings of details and outline drawings, not sections, of many of the vases in conformity with volume I of the same work. The description and discussion of the vases are by Sir John Beazley whose loving care for detail never eclipses keen appreciation of the vases as works of art. Characteristically too the book is more than a catalogue of pieces and includes detailed studies on relevant subjects of myth, genre, vase shapes and painter's style making it a real contribution to the study of Greek painting and archaeology, and not simply documentary. Dates are assigned to each of the 48 vases figured and a noteworthy feature is the frequency of cited parallels in gem engraving, a branch of Greek art much neglected of late.

Price and content might suggest that this is a book only for the connoisseur, and that no collection catalogue can have any ' unity ' of interest, but these impressions are false. Too often our knowledge of the decoration of Greek vases derives from their display cheek by jowl in a museum show case or small half-tone reproductions which reduce them to the status of geological specimens : but these drawings and photographs offer the nearest approach to personal inspection of the individual pieces in the best light (and comfort) and as such can appeal to a somewhat wider public. And the unity lies in the excellence of the vases, which, if not all masterpieces, are all of the best, The star is the Pan Painter's name-vase, a bell crater figuring on one side the death of Actaeon and on the other Pan's pursuit of a shepherd boy, a vase of which Beazley once wrote . . ' there is no finer vase in Boston, there is no finer vase anywhere '. Handling the life size photographs of this masterpiece is quite the next best thing to handling the vase itself, and perhaps almost as completely satisfactory. JOHN BOARDMAN.

MOGOLLON CULTURAL AND CONTINUITY AND CHANGE. THE STRATIGRAPHIC ANALYSIS OF TULAROSA AND CORDOVA CAVES. *By* PAUL S. MARTIN, JOHN B. RINALDO, ELAINE BLUHM, HUGH C. CUTLER, *and* ROGER GRANGE, JR. *Fieldiana : Anthropology*, vol. 40 (*Chicago Natural History Museum*). 528 *pages*, 179 *illustrations*. *Chicago*, 1952. $8.

The rough and mountainous, largely semi-arid, region of the South-west (most of New Mexico, all of Arizona, and southern Utah and south-westernmost Colorado) has long been the archaeologically most popular section of the United States, and is perhaps the archaeologically best known. The ancestors of the picturesque modern Pueblo Indians and of other groups left more striking remains—notably, large stone buildings and attractive painted pottery—than the Indians of other parts of North America north of Mexico ; and for many years they have excited interest and undergone extensive investigation and intensive study.

Yet there are still many unanswered questions—even perhaps unrecognized problems —in south-western prehistory. For the past twenty years, a major topic of study and of debate has been the relationship between the ' Anasazi ' pueblo sequence of the northern south-west and the ' Mogollon ' sites to the south, first distinguished from the ' Anasazi ' by the late H. P. Mera in 1934 and, more definitely, shortly thereafter, by E. W. Haury, H. S. Gladwin, and others, most of whom have considered ' Mogollon ' as very ancient and quite separate. A major deficiency for proper consideration of the subject has been the absence of perishable materials of the early Mogollon group from controlled excavations, to compare with ' Basketmaker ' (early Anasazi) collections from the dry caves of north-eastern Arizona and south-eastern Utah, and to analyze for understanding Mogollon culture.

Drs Martin and Rinaldo of the Chicago (Field) Museum have long been active in Mogollon studies, producing a series of important reports on field work in the Reserve district of south-western New Mexico. In their 1950 excavation in Tularosa Cave, in that area, they remedied this lack of artifacts of vegetal materials and produced a much more nearly comprehensive picture than previously available of Mogollon technology and material culture.

The report describes, concisely with much use of tabulation, objects found in Tularosa Cave and in the nearby, less outstandingly important, Cordova Cave, investigated in 1951 : pottery (none in the very deepest levels) ; a variety of stone implements, bone and antler objects ; cordage (largely yucca-fibre and cotton), sandals, cotton cloth,

basketry, and other textile objects ; a great variety of wooden specimens, and miscellaneous items ; two human burials—mummies—tightly flexed ; feathers, animal bones, plant remains (maize, beans, *Cucurbita pepo*, cotton, white-flowered gourd ; and many local wild plants, notably sunflowers).

In a 25-page ' summary and conclusions ', Dr Martin briefly reviews the material, discussing the dating of the sites (Tularosa Cave : ' about 300 B.C. to A.D. 1100 ') and of the six successive cultural phases represented, and tabulating the artifacts under these phases ; and points out major observable trends and changes, in cultural development, and in bases of subsistence. Incidentally, the surprisingly ancient occurrence of Indian corn (*Zea mays*) is confirmed by that found at Bat Cave, New Mexico, not far to the north-east. Distinctions in detail from Anasazi (Basketmaker-Pueblo) culture and general resemblances over large areas are pointed out ; and interesting speculations are offered on the anthropological significance of the material.

Additional material from other sites in the same area, largely but not entirely of late phases (about A.D. 1000–1300), investigated in 1952, have been published separately : P. S. Martin, J. B. Rinaldo, and Elaine Bluhm, *Caves of the Reserve Area*, Fieldiana : *Anthropology*, vol. 42, Chicago, 1954 (227 pp. 102 illustrations). Of special interest in this smaller, supplementary volume are the specimens which support the indications at Tularosa Cave that the bow and arrow reached the south-west earlier than has been thought—before A.D. 500—although here, as in the northern south-west, actually superseding and replacing the spear-throwing stick only about A.D. 700.

Detailed studies by specialists on the maize and other plant remains from Tularosa Cave, on the human skeletons, and other special topics, are awaited with interest, as well as a comprehensive general discussion by Drs Martin and Rinaldo of the Mogollon culture which they have done so much to bring out of the past, through excavations in these caves and at the S U pit-house village and other sites in the open, and through admirable, prompt and systematic publication of their findings.

A particularly admirable report on a Mogollon pit-house village-site, near Point of Pines in eastern Arizona, has recently been published : *Crooked Ridge Village*, by Joe Ben Wheat, University of Arizona Social Science Bulletin No. 24, Tucson, Arizona, 1954 (183 pp. 87 illustrations). A definitive general study of the status and relationships of the Mogollon culture by Dr Wheat (now of the University of Colorado) has just appeared as a memoir of the Society for American Archaeology and the American Anthropological Association, *Mogollon Culture Prior to A.D.* 1000, WHEAT, 1955.

I cannot conclude this combined review and bibliographical discussion without another reference to Dr Emil W. Haury, under whose direction I first became acquainted over twenty years ago with the Mogollon problem, and his important publications too numerous to list.

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THE KALAHARI SANDS OF THE LUNDA (N.E. ANGOLA) THEIR EARLIER REDISTRIBUTIONS AND THE SANGOAN CULTURE. *By* J. JANMART, *Museu do Dundo, Companhia de Diamantes de Angola, Dundo. Published in Lisbon*, 1953. 2 *maps*, 34 *text figures and plates.*

In this publication we welcome another monograph by the late Mr J. Janmart on the Kalahari Sands of north-eastern Angola where the diamond workings have exposed deep sections through these Sands such as can rarely be seen in any other parts of the continent that are covered by them.

The book may be divided into two parts, the first dealing with the geology and the second with certain aspects of the prehistoric content of the later sands. A useful map shows the positions of the sites mentioned in the text.

After a definition of terms, a description of the three morphological levels at which the Kalahari Sands are found in north-eastern Angola (2700–3000 ft.; 2400–2700 ft. and 2100–2400 ft.) and of the formations that underlie them, the author proceeds to show that the Sands of these three different levels are really of three different ages and are not all of one age. His evidence for this belief is impressive, but owing to the lithologically similar nature of the Sands wherever they occur a doubt must still remain, though a subdivision, such as the author suggests, occurs elsewhere in the Congo and also probably in Rhodesia.

The two earlier Sands have so far yielded no archaeological material though negative evidence must be treated with reserve, but from the lowest Sands (Redistributed Kalahari Sand II of the author) have been recovered Sangoan artifacts. It should be noted that these Redistributed Sands II equate with Leakey's 'Kalahari Sand *in situ*' and the Redeposited Kalahari Sands I and II of Leakey are two further and later stages of redeposition of Janmart's Redistributed Sands II. It is suggested that an agreed revised nomenclature for these north-eastern Angola Sands would be of value in order to prevent any possible later confusion of terms.

The Redistributed Kalahari Sands II of the author are Upper Pleistocene in age by reason of the Acheulian handaxes that are found below them and the Sangoan implements that are contained mainly within the lower 7–10 feet of these compact sands. Photographic illustrations are given of the main Sangoan implements found in the Sands: picks, 'tea-cosies', circular core scrapers, polyhedral stones, choppers, discoidal cores and beautifully made bifaced foliate lance heads.

During the earlier survey made by Dr and Mrs Leakey in 1948 none of these lance heads were recovered from beneath these sands and the only implements found were the cruder forms of pick and other heavy implements which led Leakey to believe that these represented proto and lower stages of the Sangoan Culture. Following further mining operations which had opened new sections Janmart recovered these and other end-products of the culture stage or stages represented in the lower levels of his Redistributed Sands II. These he suggests indicate that the proto and lower stages of the Sangoan, as far as north-eastern Angola is concerned, do not exist or still await discovery and that the assemblages previously recovered represent only the factory débris or mining sites of these Sangoan Stages, which are thus the equivalent of Leakey's Middle Sangoan of Angola.

This evidence correlates well with what is found in Rhodesia and northern Bechuanaland where the final Chelles-Acheul is replaced by regional variants of the Sangoan which typologically are of 'Middle' Sangoan form in accordance with the Congo and East African division. If as many of us believe, the regional Sangoans developed out of the Chelles-Acheul in areas where the ecological conditions are such as to dictate the adoption of numerous woodworking tools and woodland equipment, then we might expect to find that the earliest Sangoan is typologically a logical development out of the Chelles-Acheul. Such is indeed the position in Rhodesia and Bechuanaland and such also is the indication in Angola as a result of Janmart's recent discoveries. If on the other hand the Sangoan is derived from quite a different origin as has also been suggested then it would be reasonable to suppose that stages indicative of the earliest beginnings of such a cultural tradition will be found, possibly contemporary with the later Chelles-Acheul, but typologically and technically very distinct therefrom. It remains for future work to provide the answer. Perhaps both hypotheses are correct for one might well expect the

cultures of the equatorial tropical rain forest areas to be early differentiated from those of the grassland and park savannah areas, so that the regional Sangoans of these areas may well be derived from two or more parallel developing traditions.

The monograph is well produced in the manner that we now associate with the publications of the Dundo Museum.

The sudden death of Jean Janmart during the earlier part of this year has deprived African prehistory of one of its ablest field-workers, and has taken from us a friend and colleague who will long be remembered with affection by all those who had the privilege of knowing him.

J. Desmond Clark.

SIGNES GRAPHIQUES SOUDANAIS. *Par* Marcel Griaule *et* Germaine Dieterlen. *Hermann et Cie.*, 6 *Rue de la Sorbonne, Paris*, 1951. *pp.* 86. 500 *francs.*

This work has five parts, of which part I, ' Systèmes graphiques des Dogon ', IV ' Les signes graphiques de la circoncision Bambara ', V ' Signes graphiques des Bozo ' are by M. Griaule ; and II ' Signes d'écriture Bambara ' and III ' Signes des Keita du Mandé ' are by Mademoiselle Dieterlen.

It is a study of the signs, some of them secret and all of them probably of magic significance, used by tribes in the great bend of the Niger in the French Sudan. Among the Dogon some signs apparently have an esoteric meaning, based on an advanced cosmogony. One wonders whether this, if it has been correctly understood, has not its roots in medieval philosophy. These signs have nothing to do with the tribal brands of the eastern Sudan, but a few signs used in non-Moslem ritual in Darfur may be of similar nature.

This book should be of considerable interest to some anthropologists, but it is of no importance to archaeologists. An Egyptologist notices a sign for ' water ' very like the hieroglyph for that element, but it is probably a case of independent invention. The sign on p. 22 representing ' God after he had created the world ' and with some additions on p. 24, explained as a person carrying the water or ' life of the world ', might on this bend of the Niger nearest to Egypt be a survival of the *ankh* sign, of great magic and ritual importance in ancient Egypt. But it does not get us much farther if it is.

A. J. Arkell.

LULLINGSTONE ROMAN VILLA. *By* G. W. Meates. *Heinemann*, 1955. pp. xvi + 168, *with* 47 *plates and* 18 *line-drawings.* 21*s.*

' The archaeologist is digging up, not *things*, but *people* '. Had a motto been chosen for this book, Sir Mortimer Wheeler's now familiar dictum would have met the case appropriately. Here the story of a very notable Romano-British house is told from start to finish by one who, having first read the evidence stratigraphically, sat down to interpret it humanistically—who has really sought to see with the eyes and think with the minds of those three Romanized or Roman families that successively (1) shaped, lived in, and then relinquished the Lullingstone Villa ; (2) took it over, reshaped it, and in a few years abandoned it ; (3) about eighty years later re-occupied it and gradually remodelled and transformed it, until it assumed the character in which it met its final fate. Fortune was kind in preserving intact the dramatic relics of this Villa's economic, social, cultural, and religious life—the water-dock (?), the quadruple grand staircase, the marble busts, the floor-mosaics, and the Christian murals—for a modern, scientific excavator to disclose. In Colonel Meates the site has found a digger worthy of its treasures (so far as one who has played no part in the actual excavation can judge) ; and his book, the fruit of science wedded to imagination, could have been a model for archaeological writing of a popular

and purposely attractive kind, had it been penned with greater care and caution. No layman, however innocent of archaeology, could fail to grasp, from the vivid and frank account of how this excavation was conducted, both the excitement and the rigorous discipline involved in digging; or to realize that sensibility in matters of landscape, art, and history is as much a part of the archaeologist's equipment as technique. The author writes with zest and fluency, avoiding professional jargon. His narrative lives, and will certainly grip the interest of every type of reader.

The term 'popular' will not grate on Colonel Meates's ears, since he explains in his preface that he was writing, not to 'instruct the expert', but to please (and, we may add, inform with fascinating detail) the ordinary interested reader; and it is in this connection that one major general criticism must be made. Colonel Meates is himself an expert; and it is surely a historian's and an archaeologist's duty to educate his public in the fundamental principles of evidence—to teach it to discriminate between a situation in which proof has been obtained and one in which the state of our knowledge falls just short of it. The most important case in point which this book raises is that of the meaning of the two rooms adorned with mid-fourth-century Christian paintings. On p. 130 and elsewhere the larger of the two rooms is described categorically as a chapel—'for chapel it must have been'; and on p. 141 it is urged that only the 'purist' will object to calling the room *tout court* a place of worship (cf. p. 142). It is, indeed, abundantly true that all the evidence points vigorously in that direction—the size and elaboration of the Christian paintings, the repetition of the Sacred Monogram (in each case irrefutably established), the isolation of the rooms, on the walls of which these pictures appear, from the neighbouring apartments to the south, and the continued use of the Christian rooms for roughly twenty years after the kitchens and baths had ceased to function. But until we can wring from these rooms undeniable proof of liturgical worship, such as the Dura-Europos house-church furnished, we cannot wholly exclude the possibility that they were, in origin, at least, merely living-rooms decorated for Christians. The *Chi-Rho* in the otherwise completely pagan pavement from the Frampton Villa, Dorset, does not make that room a chapel. Pagan religious paintings on the walls of a Pompeian dining-room or bedroom do not make that room a temple, any more than 'holy pictures' or religious statues in a Christian home create a church. Can only a 'purist' be expected to appreciate the difference between a very high degree of probability and certainty? To have acknowledged this difference frankly would not have weakened the very strong case for detecting in the Christian rooms at Lullingstone a British house-church (pp. 143, 145, 146); and it would have disarmed the sceptics.

The second point is that of the position of the *Chi-Rho* monogram at the west end of the south wall of the presumed chapel. On p. 140 we learn that a large fragment of painted plaster depicting a column-cap has attached to it part of the room's south-west angle, and that two contiguous columns flanked the bewreathed Monogram on the right. [What the plaster actually shows is (*a*) a column-cap painted on a fragment of which the left-hand edge seems to join onto another fragment carrying a portion of the wreath; (*b*) two fragments each painted with part of a column-shaft and leaves of the wreath just to the left of it; these may well belong to (*a*); (*c*) various fragments fitting together which seem to show portions of two contiguous column-shafts: but these are not linked with leaves or column-caps; (*d*) the cap and upper portion of the shaft of a painted column on the fragment from the south-west angle of the room (pl. 45), which does not at present link up with anything]. We next hear that the column-cap from the corner of the room is '*taken* [reviewer's italics] to be that of the taller [there is no proof that it was taller] of the two columns on the right of the wreath', since its design and

colours are identical with those of the cap of the inner column, and that hence ' the position of the Sacred Monogram is secured '. It is true that many of the floral fragments came to light near the west end of the south wall of the Deep Room (p. 130). But is the position of the wreath and *Chi-Rho* really secured until we know for certain that there were two adjacent columns immediately to the right of the wreath and until the angle column-cap has been safely fitted to the outer member of such a pair of column-shafts ? It is, indeed, very probable that two contiguous columns did flank the right side of the wreath and that the outer one carried the cap from the angle—particularly *if* the ' set-back ' on the south wall of the Deep Room (see fig. 13 (a)) was repeated on that of the Christian room above it, leaving but a short space at the west end for holding the Chi-Rho-cum-columns design. But as yet the final links in the proof are missing.

Thirdly, as regards the figured paintings that possibly adorned the ' chapel's ' east wall—twice it is stated as a fact (pp. 133, 146) that the filling at the east end of the Deep Room, beneath the modern road, ' contains this painted plaster '. This may very well be so ; and it is infinitely worthwhile expending money, time, and labour in searching for it. But at present we cannot tell whether the plaster fragments, of which the edges peep tantalisingly out from the filling, were not shot there from the as yet unreconstructed paintings of the south wall, or, indeed, whether they are even painted. To quote the author's own words (p. 147), ' it does not do for a responsible archaeologist to make sanguine forecasts '.

Fourthly, there are Colonel Meates's two concluding paragraphs. Naturally he would be the last to claim that the picture which he paints there is founded on established fact. Nevertheless, it breathes an air of certainty that, to the untrained mind, might well be misleading. How do we know that the Villa's violent end by burning was the deliberate work of hostile barbarians ? Could it not equally well have been the result of accident ? In a room adjacent to the ' chapel ' a ' paint-box ' was discovered, burnt and crushed in the final conflagration. Colonel Meates suggests that someone was engaged at that moment in touching up the Christian paintings. Is such a scene convincing, when Saxon marauders were raging through Kent ? Lullingstone lies some distance from the Thames, and news of their presence in the country is likely to have outstripped the actual onslaught. The final fire is certain : its cause is at present guess-work.

The reviewer has listed a large number of small inaccuracies, inconsistencies, and obscurities, in matters both of fact and of interpretation, in the text and in the figure-captions. These cannot be printed here. Of the line-drawn reconstructions of the Christian paintings it must be remarked that they often do not tally either with the author's text or with the evidence of the original painted plaster. Of the plates and their captions several criticisms could be made. Here two questions only can be raised. Why was it necessary to clip off the left-hand side of the pots on pls. 14 and 22 ? And why has the author reproduced on pl. 42 an out-of-date photograph of the figure in the second intercolumniation from the left on the west wall of the ' chapel ', taken before the right hand was fitted in (*cf.* p. 132) and with the hand now known to be that of figure No. 5 (cf. pl. 46) appearing incorrectly in the upper right-hand corner (the figure shown in pl. 43 is No. 3) ? A more complete and accurate photograph has long been in the hands of the present writer.

All these points of criticism have been set out in the hope that another version of *Lullingstone Roman Villa* will eventually appear, in which over-confident assertions could be modified and in which errors and defects could be removed, as well as new evidence incorporated. Whether it was really proper to present the first connected account of

the site in popular form, or to write a book in any form, before the excavation was completed, is a debatable question, as the author himself appreciates (p. vi). Nevertheless the book is here, and it must be observed that its value could have been increased by the inclusion of at least some simple notes (publishers are mistaken in imagining that such things scare the 'general reader') and of a short, but serviceable, bibliography (as well as of a more complete index). To some readers this may well be their first introduction to the details of Roman Britain and of Roman imperial life and culture; and they would certainly have welcomed suggestions for further reading on such subjects as Roman Britain as a whole, Romano-British villas, Roman mosaics, Roman portraits, Roman pottery, Roman and proto-Christian sculpture and painting. The Lullingstone Villa is of far more than merely local interest: some of its finds are, indeed, of empire-wide significance; and references would have greatly helped to emphasize this highly important aspect. The documentation needed for a popular account and that required for a specialist report are manifestly very different. The latter does not render the former unnecessary—assuming that there are particular excavations meriting general as well as specialist treatment and that Lullingstone is one of them. J. M. C. Toynbee.

THE LATIN CHARTERS OF THE ANGLO-SAXON PERIOD. *By* F. M. Stenton. *Oxford: Clarendon Press* (*London: Geoffrey Cumberlege*). 1955. viii+103. 10*s* 6*d*.

This book presents an expanded version of three lectures given by Sir Frank Stenton at King's College, London, in 1954, as a general introduction to the study of Anglo-Saxon royal charters. In a short preface the author states his purpose of treating these documents from a historical rather than a diplomatic point of view, and of tracing the development of the charter as a formal record of a grant made by the authority of the king in his council. The discussion is limited to royal grants of land or privilege, sometimes of both; of such records there exists an almost unbroken series from the 7th century to the Norman Conquest, and thus these form ample material for the purpose.

Following the pattern of the original lectures on which it is based, the book is divided into three parts. The first, which is concerned with charters and their criticism, begins with a short history of charter-criticism from Leland to the present day, in which due tribute is paid to the work of many scholars, in particular Kemble, Stubbs, Birch and Stevenson. The author remarks with truth that 'it is with Stevenson that the modern study not only of Anglo-Saxon diplomatic but also of Anglo-Saxon history really begins'. Yet notwithstanding the valuable contributions made in recent years by Dr Harmer, Dr Whitelock and Miss Robertson, the fact emerges that much work still remains to be done before the whole corpus of Anglo-Saxon charters is properly edited; the difficulties of rightly deciding whether a given charter is genuine or not have increased since the days of Kemble. The various methods of testing the authenticity of charters are fully described, and while comparison of the formulas with those of other documents of the appropriate period must still form the basis of charter-criticism, it is clearly shown that a suspected document should not be rejected on diplomatic tests alone, for these can lead to inconclusive results, and historical or linguistic evidence can often be the deciding factor.

The second part treats of the development of the charter in form and in substance. The pre-history of the charter lies in the old Roman private deed; and it is suggested that the introduction of the charter into England may have been due to Archbishop Theodore, since the earliest examples date from his time and are of Kentish provenance. The gradual growth of the charter from these bare beginnings, which consist almost

entirely of factual matter, to the elaborate and artificial productions of later centuries is discussed with much detail, but their importance as evidence for social history is constantly emphasized. A lively account of the deterioration, after Offa's time, of the official Latin of these charters and its replacement by English is given; from the evidence adduced, it would appear that in the 9th century, when litigation became common, the clerks, whose Latin was only just equal to the composition of an ordinary charter, found themselves called upon to compose elaborate legal records, a task which was beyond the scope of their Latinity. Yet the formal Latin diploma survived because of its solemnity and the idea of permanence which it conveyed. The historical importance of 9th century charters is strongly emphasized, and particular mention is made of those which specify the exemption of estates from public burdens; it is also about this time that the difference between *lænland*, i.e. an estate of which the tenure is restricted to a lifetime or a short succession of lives, and *bōcland*, i.e. the estate secured to its possessor's heirs by royal charter (whence the not infrequent place-name *Buckland*), becomes of fundamental importance.

The last part, entitled ' The Last Century of the Solemn Charter ', deals mainly with matters of historical interest on which the charters throw light. Though there exists a large number of 10th century and early 11th century charters, these are somewhat disappointing when compared with their predecessors, although they are of great interest to the topographer and (in the case of Athelstan) the political historian. Their chief value, apart from topographical interest, is found to lie in the evidence they afford for the distribution of settlements in southern England and the light which they throw on the agrarian and fiscal systems of the time. The charters of Athelred II, of which three are quoted at length in translation, are shown to be often of great importance not only as records of salient historical events, but also as illustrations of the relationship between the king and his leading subjects, and they sometimes attain the length of a full-scale narrative. With Cnut and his successors the charter becomes less extensive, and the justification of the king's actions is sometimes omitted; it is suggested that this may have become out of date in a régime established by conquest, but more work remains to be done on this period. A late charter of Edward the Confessor, which was once considered as evidence that the office of chancellor existed before the Conquest, is shown (pp. 86–7) to be a forgery. Finally, with the advent of the Norman kings, the charter, already obsolescent, is ousted by the shorter and more convenient writ.

To sum up, this book is a thoroughly sound and scholarly exposition of the history of the Anglo-Saxon charter, and deserves the warmest welcome. The value of charters as evidence for the social and political history of their time is indeed never lost sight of, and the presentation and discussion of the evidence is clear and concise. It is no small feat to have achieved so much within the limits of a hundred pages.

K. R. Brooks,
Southampton University

THE TEMPLES OF KAWA II. The History and Archaeology of the site. *By* M. F. Laming Macadam. *2 vols., text and plates. Published on behalf of the Griffith Institute, Ashmolean Museum, Oxford, by Geoffrey Cumberlege, London.* £8 8*s*.

In this book Dr Macadam completes the publication of the excavations conducted by the late Professor F. Ll. Griffith at Kawa in the Sudan in 1929–31. The results of a subsequent season directed by L. P. Kirwan in 1935–6 are also included.

In Part I Dr Macadam published all the inscriptions found at Kawa apart from texts on the walls of temples. In this new part he describes in detail the temples and, where

it is possible to do so, indicates how and where objects were found. Because of the inadequate nature of the field notes available to him, the result is not an archaeological report in the strictest sense, but rather an epigraphic survey of the site with additional notes on objects. The unfortunate results of this method of publication have been largely avoided by the addition of a very full catalogue of objects found ; it includes all the details which could be discovered from the excavation notes and much more besides in the way of additional information. One may regret that the ultimate home of many objects is accompanied by a question mark ; a little extra labour would have confirmed that objects 0620, 0606, 0807, 0809, 0572, 0545, 0549, 0458, 0394 are all in fact in the British Museum. Furthermore the following objects, the whereabouts of which are given as unknown, are in the British Museum : 0623, 0808, 0314, 0152; the small sandstone ram's head 0548 is in the British Museum, not the Khartoum Museum.

A most interesting chapter on the chronology of the Temples provides much information on the history of the site which should be of great value to the historian of the Sudan and of Egypt's later Dynasties. The early history of the site is obscure. An inscribed statuette of late Middle Kingdom date suggests that the place was already settled by the Egyptians in the Middle Kingdom—a fact made the more probable by the existence of an Egyptian station as far south as Kerma as early as the VIth Dynasty. Macadam thinks that further excavation as Kawa might reveal traces of early building beneath the temples already excavated. At the present time the most positive evidence points to the founding of the town or of one of the temples by Amenophis III. The ancient name of the place, Gematen or Gempaten, should imply a foundation by Akhenaten, but no trace of his reign has been found on the site. Macadam thinks it possible that any building there by Akhenaten was removed during Tut'ankhamūn's reign. The problem is not easily solved, however, as the author admits, and the survival of the name Gematen, perpetuating the memory of the hated Aten, is most peculiar.

Temple A, which may have been founded by Tut'ankhamūn, has traces of the work of several of the Ramesside Pharaohs, after the time of whom the site seems to have been surrendered to invading sand until the time of the Ethiopian Kings of the XXVth Dynasty. Above all Taharqa lavished much attention on Gematen. He cleaned and restored Temple A and began an entirely new temple, Temple T. The most remarkable thing about this second temple is the relief decoration on the walls. Taharqa imported artists and workmen from the region of Memphis who produced copies of the reliefs of the Old Kingdom valley temples of Abusir and Saqqarah. The degree to which this copying was carried is remarkable. In one scene three Libyans are given the same names as they have in the parallel scenes in the temples of Sahurē' of the Vth Dynasty and Pepy II of the VIth Dynasty eighteen centuries before. A most interesting scene from this temple is the representation of the temple orchestra of trumpeters, drummers, harpists and singers, now unfortunately destroyed by rain. It was in the Hypostyle Hall of Temple T that Taharqa erected the small shrine which now occupies the central position in the Egyptian Sculpture gallery of the Ashmolean Museum.

The third important monument at Kawa is Temple B, a small much ruined structure the date of the foundation of which is not known. It contains work done either in the late Napatan or Early Meroitic periods. The scenes and inscriptions on the walls are not of great interest ; many of the hieroglyphic inscriptions are badly confused and often almost meaningless. Nevertheless all that remains is here carefully described and, where possible, translated.

In a section additional to his discussion of Temple T, Dr Macadam attempts, with considerable success, to reconstruct the original dimensions of the building. This

attempt was inspired partly by the remarkable similarity not only between the Old Kingdom scenes in the temple and in their original forms, but also between architectural elements like columns at Kawa and at Abusir. In the course of his study Macadam found that there was a remarkable consistency in the representations of kings and gods in the standing position. His findings accord closely with the results of a study of the standing male figure recently published, by Iversen in *Canon and Proportions in Egyptian Art*. Macadam's method of measuring is not the same as that used by Iversen, but his results form a remarkable confirmation of Iversen's theory according to which the same system of proportions, based on the Egyptian system of linear measurement, continued in use from the Old Kingdom to the beginning of the XXVIth Dynasty.

The account of the temples is followed by a report by L. P. Kirwan on the season of excavation carried out at Kawa in 1935–6. In addition to reclearing sites dug by Griffith in the earlier season, three inhabited sites in the temple area were dug. Few objects of great interest were found, but an important result of this additional excavation was the conclusion that Kawa could not have been burnt by the Roman Prefect Gaius Petronius in 23 B.C., as Griffith originally thought. New evidence now points to the destruction of the town in probably the third century A.D., although the destroyers, whether they be Blemmyes, Axumites or the Nōba, remain obscure.

In preparing this second part of the report on the excavations at Kawa, Dr Macadam has overcome great difficulties in working from material which by itself was never intended to form the basis of a publication. He has produced a most adequate description of the site with most of the information which someone working on the material finds will want. His historical notes are full of interest, not least of all his second thoughts in the Postscriptum. The Text volume is accompanied by a volume of line and photographic plates of good quality. T. G. H. JAMES.

QATABAN & SHEBA : Exploring Ancient Kingdoms on the Biblical Spice Routes of Arabia. *By* WENDELL PHILLIPS. *London*. 1955. *Bound. In 8vo, 335 pp., illustrated with 82 photographs, 5 diagrams and 3 maps. Victor Gollancz Ltd.* 21*s*.

This book deals largely with the author's rich archaeological discoveries in South Arabia ; first in the Aden Protectorate, then in Yemen, finally in Oman. But other topics of antiquarian interest are brought in, such as an account of Mount Sinai with reference to the manuscripts in Saint Catherine's Monastery. The text and fine photographs give a vivid cross-section of Sabean and Qatabanian antiquity. Moreover the fascinating narrative is of permanent value as a record of mid-twentieth century South Arabia.

The Sabean custom of depositing dedicatory inscriptions in the temple (p. 268) is paralleled at Ugarit. Ugaritic texts §69 and §70 are such dedicatory stelae, and the Legend of Aqhat (2 *Aqht* I, 27, 45 ; II, 16) tells us that it was a standard duty of a good son to erect such a stela in the temple for his father. (For the texts and translation, see my *Ugaritic Manual* and *Ugaritic Literature*, respectively). Parallels between South Arabia and the Fertile Crescent are often due to the fact that the Syro-Arabian Desert was not always a barrier between the two areas ; caravan trade and tribal migrations often and in many ways linked the two areas. The historical background in the book is not as a rule based on sources personally controlled by the author. He copies quite a few unfounded opinions such as the late domestication of the camel (p. 107), against the testimony of all the sources : written and archaeological.

The problem of coping with the local and official psychology of Yemen was too much for the expedition, so that the excavation at Marib came to grief. Even the

squeezes, made at the cost of so much time and labour, had to be left behind when Phillips and his staff fled across the Aden frontier. Wendell Phillips likes people and is concerned for the welfare of his associates. He has vision and works indefatigably. His words on securing funds and expedition supplies are classical (pp. 308–18). His long list of substantial contributors (pp. 319–21) shows that he did not get one or two lucky windfalls ; his resources are the accumulation from many varied contacts, and the result of hard, persistent and intelligent labour.

The test of the author's vibrant spirit was his decision to go on working in unknown Oman, after his fiasco in Yemen. In the hands of an ordinary mortal, the story would have ended with failure in Yemen. Under Phillips' leadership, it ends instead with triumphant discovery in the Dhofar province of Oman.

Don't fail to read *Qataban & Sheba*! It is an engaging and honest book, revealing the man as well as his work. CYRUS H. GORDON.

A HISTORY OF THE SUDAN TO A.D. 1821. *By* A. J. ARKELL, with a Foreword *by* SIR HAROLD MACMICHAEL. *University of London, the Athlone Press*, 1955. 8 *in.* × 5½ *in.*, 249 *pages*, 24 *half-tone plates*, 27 *text figures*, 11 *maps*. 21*s*.

There has for some time existed a need for a book of this kind. A good deal of excavation and historical research has been carried out in and on the Sudan during the pasty fifty years, and the results of most of it have been published—not always very promptly—in various journals and excavation reports. Hitherto, however, there has been no comprehensive publication in which all these results have been collated and summarized. Mr Arkell has now satisfied this need and has successfully accomplished the difficult task of condensing the available information into a single volume of handy size and comparatively modest cost. It covers the history of the country from prehistoric times to the Egyptian conquest of the early 19th century. It is primarily intended for general readers, especially those in the Sudan, and in his preface the author modestly expresses the hope that the book will provide a framework for the history textbooks the Sudanese schoolmasters will now be able to compile.

Now although the course of the main stream of this history is reasonably well defined, the source of some of its tributaries is obscure. In other words, many problems remain to be solved. Who, for instance, really were the so-called C-group people ? Who were the tribes referred to in the ancient Egyptian inscriptions which mention trading and punitive expeditions to the Sudan ? What was the origin of the Kings of Cush who conquered Egypt about 725 B.C. and reigned there for a time as the XXVth Dynasty, until forced to retreat to the Sudan ? Coming to later times, what was the origin of the Fung kings who came to power about A.D. 1500 and who were known far outside the boundaries of the Sudan ? Questions of this kind offer a temptation to the historian and a challenge to his ingenuity and imagination. A temperamentally cautious writer might perhaps consider that a book intended for the sort of reader envisaged in the preface should confine itself to established fact and generally accepted opinion. Mr Arkell, however, accepts the challenge. He has first-hand knowledge of Darfur and is a student of its history. He is always conscious of the wide open spaces which stretch westward from the Nile across North Africa—areas which were probably less arid four or five thousand years ago than they are now and across which people could move more freely. It is therefore not surprising that he should look for, and find, answers to some of the problems of Sudan archaeology in these western areas. He is also inclined to deduce connections between people or places from verbal similarities in their names. Thus he suggests that the ancient Egyptian *Nehesi* may survive today in the Mahas of Dongola ;

the ancient *Irtet* may have become the modern Urti in Darfur; the *Temehu* may be recognized in the Tama of Wadai, and so on. The famous journeys of the ancient caravan leader Harkhuf, may have taken him to Darfur. When the Meroitic kingdom of Cush was finally overthrown about A.D. 350, the royal family may have fled westward through Kordofan to Darfur. As for the Fung kings, although Mr Arkell in his preface rightly says that nobody really knows their origin, he nevertheless in the book advances the suggestion, on very slender evidence, that they may have come from Bornu. All these and other imaginative reconstructions are, of course, put forward impersonally as possibilities only, but is it safe to assume that the non-expert readers to whom they are offered possess the knowledge and capacity to differentiate between possibility, probability and fact? The point is, perhaps, of only academic importance; it affects what may be called the fringe areas of the history. For the rest, all the important facts and evidence are fairly set out. The story of the thousand-year rule of the Kings of Cush as written here should be of value to the specialized student. It takes account of the most recently published evidence and gives sound reasons for the modifications which now seem to be necessary in the theories which Reisner first propounded more than thirty years ago. The origin of this line of kings has not yet been definitely established, but at least it seems clear that they were not Libyan intruders as Reisner had supposed. The suggestion now is that they were in origin hereditary chiefs of Cush (i.e. native Sudanese of the Dongola region) who had become Egyptianized during the centuries of the Egyptian occupation of the Sudan. This is certainly probable.

The book is well produced and the illustrations have been judiciously chosen. The system of foot-notes adopted makes for easy reference to the bibliography at the end.

F. ADDISON.

SUMERIAN ECONOMIC TEXTS FROM THE FIRST DYNASTY OF ISIN. *By* VAUGHAN EMERSON CRAWFORD, PH.D. (*Babylonian Inscriptions in the Collection of James B. Nies, Yale University, vol.* IX). *New Haven, Yale University Press,* 1954. £3 3*s.*

The ninth volume of a well-established series, this is in the main a publication of cuneiform texts in a great and varied American collection, most volumes of which have presented material of a distinctive character. Here the two pecularities are of date and subject: the former is that age which immediately followed the collapse (in still obscure circumstances) of the Third Dynasty of Ur, the subject is transactions concerning hides, leather, and their treatment and employment. Documents of any kind belonging to this period have been remarkably few hitherto, and little has been known about the crafts and trade in leather. In both respects this volume brings a notable addition to knowledge. And although economic records of this kind, concerned with official or temple economy, have little literary form, there is a linguistic interest (as the author points out in his Introduction) in observing the shift of language from Sumerian to Akkadian. So formal, however, is the usage of the scribes who drew up these documents that one can form but little idea of the everyday speech around them. The writers, professional clerks, ' writ no language ' but an elaborate repertory of Sumerian formulae, while the usage of ordinary society was clearly evolving towards a predominance of the Semitic speech. Indeed, the preceding Third Dynasty of Ur is no doubt formally misrepresented to us as an almost wholly Sumerian-speaking people by the nearly exclusive survival of the same kind of clerkly compilations as here. These documents of the immediately succeeding generation show, in any case, the increasing penetration of ordinary language into the formulae of affairs, as it might be of English

into the Latin or Norman-French of medieval lawyers. Nevertheless, Isin being a southern city, it is probable that, about the beginning of the 2nd millennium B.C., the majority of its citizens did in fact still use Sumerian, doubtless with borrowings of Semitic vocabulary.

As now usual in publications of this kind, the cuneiform texts (93 plates) are preceded by introductory matter, here extending to some 70 pages. The space allowed to discussion of the contents and order of the date-formulae, even if out of proportion, is justified by the new material, and the Indices (of persons, gods, buildings, occupations, and places) are what is expected in such publications; these are replete with information, although a number of mistakes and misapprehensions have been noticed. Perhaps the most interesting feature is the prominence of the people called Mar-tu, with their distinctive functions and names, no. 316 being especially notable in this respect. But the Catalogue, of 30 pages, may fairly be called disappointing: apart from statistics, it gives only the briefest general description of the subjects followed by a list of 'Key Words' which, not concerning themselves with the matter of the tablet, indicate no more than its formulae; further an Index of these gives no more help. Nor is this much mended by another 'Index of the Subject Matter', which is listed only by leading words in English, without reference to the Sumerian thus translated, the only help being a short statement on pp. 40 f. It is much to be hoped that the Dissertation which the author has written (see p. 3) upon the contents and underlying material of these novel texts may yet be published, for it is a sad waste that students of this important collection should have to regain by individual study information which has already been won by the author in the course of his work.

C. J. GADD.

STRATIFIED CYLINDER SEALS FROM THE DIYALA REGION. *By* H. FRANKFORT. *University of Chicago Press.* [*Oriental Institute Publications, vol.* LXXII]. $20.

Engraved seal-stones are one of the most useful groups of artifacts for reconstructing the life of early periods because they are fairly common and usually found in good condition; and they are particularly important for the study of ancient cults. Their abundance is the result of their indispensability in a society where commerce flourished and it was necessary to establish the ownership of goods. The late Dr Frankfort, whose recent premature death is so grievous a loss to oriental studies, was more aware than many of the importance of dating by sound stratigraphic methods; and this fine book is an attempt to check typological classification by them. It thus supplements his classic work on cylinder seals, published in 1939, which traced their stylistic development throughout the period of their use. The gist of the conclusions is presented in a short Table (No. 1, p. 11). The discovery of seals of an earlier period in later strata is interpreted as due to their survival in use and not to continued manufacture. It is, of course, a truism that strata are dated by the latest, not the earliest, of the objects found in them; and that to date them by single objects, particularly those of durable material, is always most dangerous. The table shows that even groups may be misleading, if used by themselves, for nearly half as many (121) Jamdat Nasr seals were found altogether in the sites discussed in the (Later) Early Dynastic strata as in the (preceding) period of their manufacture (Protoliterate c-d).

There are 78 pages of text in which first principles of stratigraphy and style are discussed (pp. 1–6). Then follows an account of the stratification of the seals in the first sites chosen for analysis (Khafajah, Tell Asmar, Tell Agrab, and Ishchali). Next comes a chapter on the stylistic development of Mesopotamian glyptic as illustrated by

these seals from the four sites mentioned, all of which are in the Diyala region. Finally comes a most interesting analysis of the subjects of the seal designs, classified under periods and beginning with the Early Dynastic, when scenes superseded geometric patterns. These include the God in his Boat, the Destruction of the Hydra, Ritual Marriage, Banquets, Adoration and Offering Scenes, Dairy Scenes, the Building of the Ziggurat, and the Gatekeeper. The Akkadian seals depict the Sun-God and other deities in various rôles and include an Indian seal. There is a short account by Dr Thorkild Jackson of the inscriptions (pp. 48–52) which unfortunately are not very helpful. Lastly are various catalogues and indexes.

The seals themselves are illustrated by superb photographs of 1002 impressions on 92 large plates, followed by plans of the four sites and a table correlating their stratigraphy. It is sad that the author did not live to see the publication of a book that must have entailed many months of hard work, which will be a standard book of reference for all workers in the field of Mesopotamian archaeology. The presentation of the results maintains the excellence we have learnt to expect from the products of the Oriental Institute of Chicago. O.G.S.C.

THE BUILDING OF ANCIENT EGYPT. *By* HELEN *and* RICHARD LEACROFT. *Puffin Picture Book No.* 101. *2s 6d.*

This is an ambitious effort to make the building of the houses and tombs of ancient Egypt real and understandable to children.

The method chosen has been to follow the work of the architect or chief Clerk of the Works of a Pharaoh who lived during the 14th century B.C.

The illustrations are taken from pyramid and tomb paintings and the architecture is ingeniously depicted by the device of using a black line to show where a roof has been removed to disclose an interior.

Unlike some other Puffins, this book is not written for quite young children, especially as the plans are perforce complicated ; but on the whole the attempt is successful as well as decorative, and may stimulate some people past childhood to study the material remains of the Egyptian civilization.

As the avenue of sphinxes is mentioned, but rarely shown, more about this monster which has assumed a place in the English language would be appropriate.

DINA P. DOBSON.

THE ORIGINS OF ART. *By* GENE WELTFISH. *Bobbs-Merrill Co. Inc., New York.* 300 pp., 99 *text figures.* $4.50.

This very naïve book is presumably written for very naïve readers. The title is wholly misleading, as the book is in fact largely concerned with the textile, basketry and ceramic designs of certain American Indian peoples. But the author has aims to furnish ' a good basis upon which the art specialist and the layman may begin widespread communication and understanding of people and their lives ' : it is difficult to elicit a meaning from this or from many similar pretentious passages in the book. The author's estimate is that ' nearly one hundred thousand dollars' worth of research and travel ' have gone into the book, and it is staggering to think that responsible academic bodies and foundations in America should have poured about £35,000 down such an unrewarding drain.

STUART PIGGOTT.